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Volume LIX OCTOBER 1951 Number 5

An Experimental Measurement of Utility
Frederick Mosteller and Philip Nogee 371

Labor and Output in Soviet Concentration Camps
Naum Jasny 405

A Loan Ratio for Inflation Control — *M. Bronfenbrenner* 420

Are Property Tax Rates Increasing? — *Colin D. Campbell* 434

The Life of John Maynard Keynes — *L. R. Klein* 443

Book Reviews (See inside front cover) 452

Books Received 460

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BOOK REVIEWS:

Leo Grebler, *Production of New Housing: A Research Monograph on Efficiency in Production* . . . . . CLARENCE D. LONG 456

M. K. Bennett and Associates, *International Commodity Stockpiling as an Economic Stabilizer* . . . . . C. ARTHUR HICKMAN 463

N. V. Sovani, *Economic Relations of India with Southeast Asia and the Far East* . . . . . JACOB M. VAN DER KROEF 464

Barbara Ward, *Policy for the West* . . . . . V. W. BLADEN 465

Brian Tew, *Wealth and Income: An Analysis of the Economic and Financial Systems of Australia and Great Britain* . . . . . H. P. E. JENKINS 466

F. W. Peaslee, *The Post-war Financial Problem and Other Essays* . . . . . ARTHUR I. BLOOMFIELD 467

Richard B. Tennant, *The American Cigarette Industry* . . . . . REAVIS COX 468

Adolf Weber, *Hauptfragen der Wirtschaftspolitik* . . . . . ROSS M. ROBERTSON 469

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THE JOURNAL OF
POLITICAL ECONOMY

Volume LIX OCTOBER 1951 *Number 5*

AN EXPERIMENTAL MEASUREMENT OF UTILITY[1,2]

FREDERICK MOSTELLER AND PHILIP NOGEE[3]

Harvard University

I. INTRODUCTION

THE purpose of this paper is to report a laboratory experiment that measured in a restricted manner the value to individuals of additional money income. Although the notion of utility has long been incorporated in the thinking of economic theoreticians in the form of a hypothetical construct, efforts to test the validity of the construct have mostly—and in many cases necessarily—been limited to observations of the behavior of *groups* of people in situations where utility was but one of many variables. The notion of utility is roughly this: Individuals behave as if they had a subjective scale of values for assessing the worth *to them* of different amounts of commodities, and this scale is not necessarily merely a stretching or a translation of the "physical" scale in use in the market place. First, there is the question of whether utility can be measured under any circumstances, even in a laboratory situation, and, second, if utility can be measured, can it be used to predict behavior in slightly different situations from those in which it was derived. It should be remarked that, in advance of this experiment, there was no assurance that utility curves could be derived in the laboratory by using administratively feasible amounts of money.

Early discussion[4] of methods for constructing utility curves was given by

[1] The authors wish to express their thanks to the Executive Committee of the Laboratory of Social Relations, Harvard University, for the funds necessary to carry out this work. We wish to acknowledge the assistance of Mr. Henry Gerbrands in the design and construction of the apparatus; the contributions of Milton Friedman, L. J. Savage, and W. Allen Wallis to the discussions of this experiment; the help of Mrs. Shirley B. Robock in conducting the pilot study; and the work of Mrs. Doris R. Entwisle in assisting with the analysis of the data and the preparation of this report.

[2] This paper was presented in Chicago, Illinois, December 27, 1950, at a session jointly sponsored by the American Economic Association, the American Statistical Association, the Econometric Society, and the Institute of Mathematical Statistics.

[3] Mr. Nogee is now at Boston University.

[4] The authors are grateful to Professors Armen Alchian and Donald C. Williams for calling their attention to F. P. Ramsey's 1926 essay, "Truth and Probability" (especially the section on "Degree of Belief," pp. 166–84), available in the reprinted *Foundations of Mathematics and Other Logical Essays* (New York: Humanities Press, 1950). When the experiment began, we were not aware of Ramsey's idea for measuring degree of belief and utility simultaneously.

Von Neumann and Morgenstern,[5] and subsequently by Vickrey.[6] The direct antecedent of this experiment is a paper by Friedman and Savage,[7] and the reader is referred to their paper for an account of the historical background of utility discussions. These authors suggest that reactions of individuals to risk can be rationalized by a rather simple extension of orthodox utility analysis: individuals in choosing among alternatives involving risk behave in a way to maximize their expected utility (as contrasted with the utility of expected income). Friedman and Savage also admit (indeed, require) the possibility of increasing, as well as diminishing, marginal utility, allowing the introduction of inflection points in utility curves. Two observations are made that seem contradictory when viewed from the standpoint of traditional utility analysis—simultaneous buying of insurance and of lottery tickets. But Friedman and Savage argue that, owing to the peculiar shape of their utility curves, individuals in such cases may be maximizing their expected utilities. Hence the behavior may be consistent with the utility concept. The basic assumptions outlined by Friedman and Savage for constructing utility curves are adopted for the analysis of the present experiment. In a limited experiment such as the present one, we cannot hope to check some of their argument, such as the existence of two or more inflection points in the utility curves of some individuals, because measurement has to be restricted to a rather narrow range of money.

In addition to theoretical material and observational data, there was available a related experiment by Preston and Baratta,[8] dealing with the interrelationship of psychological and mathematical probabilities. The approach of these authors parallels the present one, in that an objective continuum is replaced by a subjective continuum, and it is likewise claimed that people behave as though responding to the subjective continuum exclusively.[9] More remotely related is a large body of data in the domain of psychophysics, where scaling problems having methodological resemblances to the present problem are encountered.

Thus, prior to this experiment, the methodology for constructing utility curves had been worked out in principle, the concept of utility curves had been extensively discussed in the literature, and some related empirical work had been published. With this information at hand, an experimental plan to measure utility was evolved which has four main steps: (*a*) to have subjects participate in a game with opportunities to take or refuse certain gambles or risks entailing use of ***real money;*** (*b*) from behavior in the game to construct a utility curve for each subject; (*c*) to make predictions from the utility curves about future individual behavior toward other and more complicated risks; and (*d*) to test the

[5] J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior* (Princeton: Princeton University Press, 1944).

[6] William Vickrey, "Measuring Marginal Utility of Reactions to Risk," *Econometrica*, XIII (1945), 319–33.

[7] M. Friedman and L. J. Savage, "The Utility Analysis of Choices Involving Risk," *Journal of Political Economy*, LVI (August, 1948), 279–304. Plans for this experiment grew directly out of discussions with Friedman and Savage at the time they were writing their paper. W. Allen Wallis also contributed to the discussions.

[8] M. G. Preston and P. Baratta, "An Experimental Study of the Auction-Value of an Uncertain Outcome," *American Journal of Psychology*, LXI (1948), 183–93.

[9] However, Preston and Baratta measure a different variable from the one measured in the present experiment, and an account of the details of their experiment is postponed to our discussion.

predictions by examining subsequent behavior toward more complex risks. It was hoped that this experimental sequence would show whether it was indeed possible to construct utility curves for individuals and, if so, whether these curves could be used to predict future behavior of the same individuals.

II. THEORETICAL DETERMINATION OF POINTS ON A UTILITY CURVE

A good description of the assumptions back of the construction of utility curves is provided by a slightly revised version of Friedman and Savage's statement.[10] The change is in the second numbered point in the following quotation. An inadequacy in the original version was pointed out by Paul A. Samuelson, but Friedman and Savage were kind enough to supply the present authors with their revision:

> In choosing among alternatives open to it, whether or not these alternatives involve risk, a consumer unit (generally a family, sometimes an individual) behaves as if (*a*) it had a consistent set of preferences; (*b*) these preferences could be completely described by a function attaching a numerical value—to be designated "utility"—to alternatives each of which is regarded as certain; (*c*) its objective were to make its expected utility as large as possible. It is the contribution of von Neumann and Morgenstern to have shown that an alternative statement of the same hypothesis is: An individual chooses in accordance with a system of preferences which has the following properties:
>
> 1. The system is complete and consistent; that is, an individual can tell which of two objects he prefers or whether he is indifferent between them, and if he does not prefer C to B and does not prefer B to A, then he does not prefer C to A. (In this context, the word "object" includes combinations of objects with stated probabilities: for example, if A and B are objects, a 40-60 chance of A or B is also an object.)
>
> 2. If the object A is preferred to the object B, then a combination of A and any object C with stated probabilities is preferred to a combination of B and C with the same probabilities, provided that the probability attached to C is less than unity. Conversely, if a combination of A and C with stated probabilities is preferred to a combination of B and C with the same probabilities, then A is preferred to B.
>
> 3. If the object A is preferred to the object B and B to the object C, there will be some probability combination of A and C such that the individual is indifferent between it and B.

To bring this discussion back to the experiment, consider the objects A, B, and C in the above quotation as

Object A: receipt of 25 cents,
Object B: neither receipt nor loss of money,
Object C: loss of 5 cents.

Most individuals will prefer A to B, and B to C. Statement 3 of the quotation suggests that, if these preferences hold and the utility rationalization for behavior is correct, there will be some probability combination of A and C such that an individual is indifferent between the A-C combination and B. Writing $U(X)$ for the utility of X, this means there must exist some probability p such that

$$pU(A) + (1-p)U(C) = U(B),$$

or, to use our monetary example,

$$pU(25¢) + (1-p)U(-5¢) = U(0¢).$$

This equation suggests that three money values be chosen in an experiment and that p then be varied until the indifference point is reached. However, the experimenters preferred to fix two money values—B and C—and the probability p, and search for an A that would bring a balance. This is done partly in deference to the view that most people are more familiar with amounts of money than with probabilities. Thus, for a particular fixed probability p_0, it is necessary to find an A such that, for a given individual,

$$p_0U(A) + (1-p_0)U(-5¢) = U(0¢).$$

[10] *Op. cit.*, pp. 287–88.

An operational definition must be set up for "indifference" or the "=" of the above equation. Let D be the object composed of a p_0 chance of A and a $1-p_0$ chance of C (losing 5 cents). Then when an A is found such that B and D are chosen *equally often*, i.e., each chosen in half of their simultaneous presentations, the individual is said to be indifferent between B and D, and the utility of B is said to be equal to the utility of D for him.

This formulation must make the reader suspect that an individual cannot tell consistently which of two objects he prefers or whether he is indifferent to them. The present experiment negates the all-or-none assumption implied by Von Neumann and Morgenstern (see quotation from Friedman and Savage, the first of the numbered properties) and supports the experience of psychologists with psychological tests showing that a gradation of preference is the rule when persons locate themselves on "physical" continua. For example, if one were offered one chance in five of winning \$5.00 against four chances in five of losing 5 cents, one would almost certainly take the bet. If one were offered the same chance of winning 1 cent against the same chance of losing 5 cents, one would almost certainly not take the bet. However, as the offer increases from 1 cent toward \$5.00, vacillation sets in, the bet is taken occasionally, then more and more often, until, finally, the bet is taken nearly all the time. There is not a sudden jump from no acceptances to all acceptances at a particular offer, just as in a hearing experiment there is not a critical loudness below which nothing is heard and above which all loudnesses are heard. This is not to deny that for many purposes and for most individuals the width of a loudness-threshold, over which the chance of hearing a click lies between, say, 1 and 99 per cent, is so small that it can be ignored. However, for certain purposes, such as constructing audiograms to assess auditory deficit, small changes may be important. The importance of this gradation of preference in utility measurement cannot be assessed until it is known to what purpose the analysis will be put. Further, as will be seen later, the width of the discrimination band may be a characteristic that distinguishes groups of people.

At best, utility can be measured only up to a linear transformation. In other words, both a zero point and a unit of measurement are assigned arbitrarily to utility scales. Since "the loss of 5 cents" and "neither winning nor losing" are the two anchoring points in the fundamental equation, it is convenient to assign them the following utilities:

$$U(0¢) = 0 \text{ utile}, \quad U(-5¢) = -1 \text{ utile},$$

where "utile" is the arbitrary name of the unit of measurement. Substituting these evaluations in the indifference equation and solving for $U(A)$, we see that, if B and D are found to be indifferent (equal),

$$U(A) = \frac{1-p_0}{p_0} \text{ utiles}.$$

For any known probability p_0 of winning A, the utility of A is known from the start from the equation just given; but A must be determined experimentally. In other words, the individual by participating in the experiment tells how much a certain number of utiles is worth to him in money. The utility scale has been assigned, and the money scale must be tied to it at different points.

What is the connection between this discussion and the mathematically "fair" offer? In a mathematically "fair" situation the money values enter directly into

the formula, or, if it is desired to translate into utilities, $U(X \text{ cents}) = X/5$ utiles, so that 10 cents is worth 2 utiles, etc. Thus the "fair" value is represented by a straight line in the cent-utility plane through the points $(0, 0)$, $(-5, -1)$. One of the points of interest in this experiment is the departure of the observed utility curves of subjects from this straight line. If an individual's utility curve lies consistently below this line, he may be called "conservative" because he requires more than a mathematically fair offer before taking a risk. Similarly, a person whose utility curve lies above this straight line might be regarded as "extravagant" because he will give his opponent the mathematically better side of the bet (see Fig. 1). Of course, if the "extravagant" person bets sufficiently often, he will soon run out of money. A person may not be in any of these three illustrative categories—he can be sometimes conservative, sometimes extravagant, and sometimes "fair."

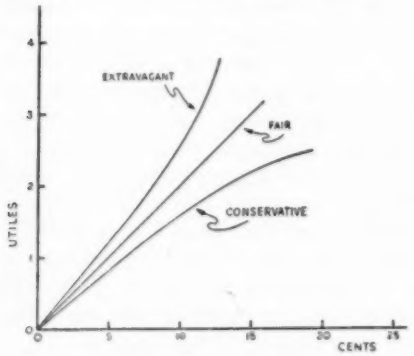

FIG. 1.—Graph illustrating possible utility curves of conservative, mathematically fair, and extravagant players for the units of measurement used in this experiment.

III. EXPERIMENTAL PROCEDURE

The experiment on the measurement of utility was conducted in two parts: a preliminary pilot study and a later experiment incorporating revisions and refinements suggested by the pilot study. The pilot study was conducted in February and March, 1948, and the experiment was conducted from February through May, 1949. The present discussion will be concerned primarily with the experiment, with only a few incidental references to the pilot study.

A. SUBJECTS

There were three groups of subjects. Two groups of five members each were composed of undergraduates attending Harvard College. They were obtained through the Student Employment Office and were concentrators (majors) in various departments of the College. A third group of five subjects was recruited from a regiment of the Massachusetts National Guard. Two additional National Guardsmen originally composed a fourth group, but these men withdrew after only part of the experiment had been run and were omitted from further consideration. One student subject left the experiment before completion because of economic and scholastic difficulties, but some of his results are included. Table 1 summarizes the group characteristics.

TABLE 1

CHARACTERISTICS OF GROUPS OF SUBJECTS

Group	Type of Subject	No. at Start of Study	No. at End of Study
A	Undergraduate students	5	4
B	Undergraduate students	5	5
C	National Guardsmen	5	5
D	National Guardsmen	2	0

Differences in economic status and personal data of the subjects were revealed through material gathered from special questionnaires. The Harvard stu-

dents were all unmarried; of the National Guardsmen all but one were married and had at least one child. The students ranged in age from nineteen to twenty-one years; the National Guardsmen ranged in age from twenty-six to thirty-four years, with the exception of the one unmarried man, who was twenty-one. All subjects had had some experience with gambling games previous to joining this experiment, although the amount and kind of this experience was not homogeneous within groups.

All but three of the student subjects were dependent upon their families for the bulk of their financial support, and the three securing the bulk of their finances independently either held large scholarships or were attending school under the G.I. Bill. The students' estimates of their annual expenditures (including tuition expense of $525) ranged from $1,200 to $2,200, with a median estimate of $1,750. The students' estimates of their parents' annual income ranged from $4,000 to $20,000, with a median of $8,000.[11] These students were atypical of the Harvard College population, since they did nonacademic work from 3 to 12 hours per week in addition to time spent on this experiment.

The financial picture of the National Guardsmen, as might be expected, presented a marked contrast to the picture for the Harvard group. All the National Guardsmen were the breadwinners for families of three to four persons, except for the twenty-one-year-old man (he contributed to his parents' support and retained only $36 of his monthly income for his own use). Income for the National Guardsmen was not dependable—two lost positions during the experiment, one falling back on unemployment compensation and the other working as a day laborer. The insecurity of their financial situations is further indicated by the fact that only one man had savings in excess of $100. Monthly incomes for the National Guardsmen were estimated by multiplying stated weekly earnings by $4\frac{1}{3}$ and adding pay received for Guard duty; these totals ranged from $162 to $254 per month. Thus the Guardsmen were supporting themselves plus two or three additional persons on very little more than the students were expending for their individual maintenance.

Besides these differences in current economic status, interesting differences between the economic aspirations of the two groups were revealed by answers to the question: "What income do you feel you would need when you're about forty in order to live comfortably?" Answers to this question are given in Table 2.

TABLE 2

ECONOMIC ASPIRATIONS OF HARVARD AND NON-HARVARD GROUPS

Annual Income Desired at Age Forty	Harvard Students	National Guardsmen
$ 2,000–$ 3,000	0	2
$ 3,000–$ 4,000	1	3
$ 4,000–$ 5,000	0	0
$ 5,000–$10,000	5	0
$10,000–$20,000	0	0
Over $20,000	3	0

In addition to large differences in the amount of income desired, different income units were used by the two groups.

[11] From Paul H. Buck, "Who Comes to Harvard?" *Harvard Alumni Bulletin*, January 10, 1948, the following data are available for all Freshmen of 1948 on students' median estimates of their parents' annual income:

	Per Cent of Total	Median Parental Income
Veteran	16	$ 7,400
Nonveterans:		
Scholarship students	27	4,950
Nonscholarship students	57	12,250

The students all stated their desired income in thousands of dollars yearly, while with only one exception the National Guardsmen stated their aspirations in dollars per week. The significance of this use of income units is problematical, but in any case this difference affords additional evidence that the economic values of the two groups are not the same.

B. PRELIMINARY INFORMATION GIVEN TO SUBJECTS

Before being hired, each prospective subject was interviewed briefly to explain the general conditions of the project. Screening was thought necessary to eliminate persons who might have scruples about participating in gambling games, but none such were interviewed. To help subjects decide whether they would be willing to participate, the following information was given:

a) The study was to be a series of experiments on the "theory of games."

b) There would be a relatively large number of sessions (approximately three sessions a week for ten weeks), and, once enrolled, subjects would be required to attend all sessions.

c) Each session would be about an hour long, and the rate of pay would be one dollar per hour.

d) The payment of one dollar would be given to the subjects at the beginning of each hour's session, and this dollar would be used to participate in the "games" that would be played. Subjects were informed that it would be possible to complete a session with considerably more than one dollar and that all money on hand at the end of a session would be theirs to keep; that it would also be possible to end up with less than a dollar, or even nothing. They would be required to remain at a session until its completion, even though they had used up their money.

e) In addition to the "games," there would be other sessions where psychological tests would be administered, and for these sessions payment would be delayed. The dollar's payment for these sessions would be put into an individual "reserve fund" for each subject, and he could be required to withdraw this money for use in certain other experiments. The subject was told that whatever balance remained in his reserve would not be paid until the conclusion of the study.

Some reassurance could be given to subjects regarding the possibility of ending up with no money (on the basis of previous experience with the pilot study), and the experimenters thought it expedient to do so. The subjects were told that in a similar study done previously average earnings of subjects had actually turned out to be approximately one dollar per hour. Unknown to the subjects, the experimenters made a commitment to the Student Employment Office and to an officer in the National Guard that, at the end of the study, they would pay any subjects averaging less than a dollar an hour an amount sufficient to bring individual total earnings up to this minimum average hourly rate. (The existence of the reserve fund made possible an unobtrusive payment.) The commitment apparently never became known to the subjects. The forecasting on the basis of the previous year's experiment turned out to be good, since the earnings of only three subjects had to be supplemented at the end of the study (by total amounts of \$2.88, \$2.17, and \$0.42 for from seventeen to twenty-one sessions).

C. APPARATUS

The apparatus used in the experiment consisted of: (1) red, white, and blue poker chips; (2) a "cage," which could be rotated, containing five dice; (3) an Esterline program analyzer for recording most of the data; (4) a series of stimulus cards; (5) a card-holder wired to the recorder, so that time of presentation of the stimulus could be noted; (6) a set of one red and one green button for each subject, wired to an indicator box in front of

the experimenter and to the recorder; (7) sheets for manual recording of some of the data by the experimenter.

The cage used for rolling dice is a standard device for playing chuck-a-luck (a three-dice game) and was purchased from a sporting-goods firm. The three original dice were replaced by five new dice, approximately identical to one another in size, weight, and appearance. The cage and dice were checked empirically for bias before the experiment started, and the distribution of results appeared random in about the proportions dictated by theory. Table 5 offers some numerical evidence on this point.

Ten recording pens on the Esterline analyzer were used for each group of five subjects, one pair of pens per person. Time appears as a linear distance between two pen strokes. For every subject on every play, the time of presentation of the stimulus card, the time of decision to play or not to play, and the nature of the decision were recorded by the Esterline. In addition, the experimenter recorded manually the nature of the decision (a cross-check), whether the subject won when he played, and, if he won, how much. The subject indicated a decision to play by pressing his green button and a decision not to play by pressing his red button.[12]

D. THE GAME

The game used in the experiment was a variation of poker dice. In this game five dice of the ordinary variety, with from 1 to 6 pips on each side, are rolled in a wire cage, and the combination of numbers on the tops of the dice is considered a "hand." Each hand can be considered an analogue of a poker hand in a one-suited deck, and so hands can be compared in terms of their relative standing in the game of poker (from lowest to highest: nothing, one pair, two pair, three of a kind, full house, four of a kind, five of a kind). The only departure from the scale of values of hands in poker was that "straights"—1, 2, 3, 4, 5 and 2, 3, 4, 5, 6—were considered of no special value and were classified among the lowest possible hands—indeed, 1, 2, 3, 4, 5 was the lowest. The highest hand is 6, 6, 6, 6, 6. There was no need to distinguish flushes. In the play of the game, the experimenters did not roll but simply displayed a hand on one of the stimulus cards. If a subject elected to play, he rolled the five dice once. If a subject secured a hand higher than that displayed by the experimenters, he won; if he secured a hand that was the same or poorer than that displayed by the experimenters, he lost. In brief, one round consisted of the experimenters' displaying a hand on a stimulus card, each subject choosing to play or not to play, and indicating his decision by pressing a button, the playing subjects individually rolling the cage once in an attempt to beat the displayed hand, and the experimenter either paying the amount of the offer or collecting 5 cents.

E. PROCEDURE OF PLAY

Chips of different colors were used instead of money for transactions during the play. The subjects were each given chips worth $1.00 at the beginning of a session, and at the end of a session all chips were refunded with money. Red chips were worth 1 cent, white chips 5 cents, and blue chips 50 cents.

As stated before, each subject had two buttons—a red and a green—in front of him. At the start of each session he was instructed to indicate his decision to play

[12] We are deeply indebted to Mr. Henry Gerbrands, of the Laboratory of Social Relations, for the design and construction of the apparatus.

or not to play as quickly as possible after the presentation of the stimulus card. He pressed the green button if he wished to play or the red button if he did not wish to play. When a subject made his decision, a light corresponding to the color of the button he pushed flashed in the indicator box in front of the experimenter. The lights in the indicator box served as visual aids for the experimenter: from the lights he could record the type of decision made and, with this immediate information concerning decisions, was able to guide the play smoothly and efficiently.

Subjects (usually five) sat around a long table where they could all see one another, the experimenter, and the stimulus-card holder at the head of the table. Twenty white chips (5 cents each) totaling $1.00 were given to each subject before the game started. A series of stimulus cards was then shown. On each card was a notation like the following:

44441
$10.00 :: 5¢

This meant: you have your choice of betting or not betting 5 cents against $10.00 that you can roll five dice once and get a better poker hand than 44441 (examples of hands that would win: 22222, 55551, 44442). The only wager permitted was one of 5 cents by each subject.

No play started until every subject had made his decision to play or not to play. If no one chose to play, the next card was presented immediately. If some or all subjects chose to play, the dice cage was passed, and each player rolled once to try to beat the displayed hand. No attempt was made to control the order in which the players rolled, so that order varied from round to round. If a player lost, the experimenter immediately collected a white chip worth 5 cents from him; if a player won, the experimenter paid him in chips the value of the offer shown on the card. All transactions connected with one card were made before the subsequent card was shown. The experimenter recorded the outcome of each person's play. Since each subject rolled independently, he played only for himself and against the experimenter (the machine) and not against any other subject. It was possible for all subjects to make exactly the same decisions, but for all to finish with different amounts of money. When questioned, the subjects stated that they felt their decisions were only slightly affected by those of the other subjects, and observations by the experimenters of the marked differences in the play of different subjects in the same group partly confirms these verbal statements.

No play started on any round until all subjects had indicated their decisions. Once one subject started to roll, the others were not allowed to change their decisions. If a subject made a mistake and pressed the wrong button, he so informed the experimenter before any play took place. To insure care, a subject had to accept the decision he had made when he pressed the button. In the analysis of results, however, his stated intent was accepted. (The decision to play or not to play was the authors' chief interest, and the winning or losing was of interest only in so far as it affected future decisions to play or not to play.) The actual number of claimed mistakes was small—less than 2 per cent for the most careless subject—and insistence on adherence to first decisions seemed to minimize the subjects' carelessness in pressing buttons. There was occasional byplay during the experiment, when subjects who were not playing took the cage to roll and see "how it would have come out if I had played." Occasionally also, a subject would re-

quest in advance that his second rather than his first roll be counted. This was allowed.

From this description of the game, it can be judged that the experimenter had many tasks to perform and that these tasks had to be performed rapidly in order for subjects not to become bored from too much dead time. Something like 1½ cards and the play connected with them, on the average, consumed about one minute. Check on the experimenter's recording of decisions was made possible through the double recording of these decisions. The subjects themselves checked the accuracy of the money transactions.

TABLE 3

TRUE ODDS FOR HANDS PLAYED

Rank of Hand	Hand	Approximate True Odds against 5 Cents
1	44321	2½¢::5¢ or 0.50::1
2	66431	5¢::5¢ or 1.01::1
3	55221	10¢::5¢ or 2.01::1
4	22263	25¢::5¢ or 5.00::1
5	55562	51¢::5¢ or 10.17::1
6	22255	$1.01 ::5¢ or 20.24::1
7	44441	$5.07 ::5¢ or 101.32::1

F. HANDS

All hands displayed on stimulus cards and their corresponding fair bets are shown in Table 3. These hands were selected by the experimenters from all possible hands after a complete table of odds had been computed. Criteria determining selection were such things as the fair offer being approximately an integral multiple of 5 cents at the lower end of the scale, and getting fairly good representation of hands of different expected frequencies. With each of these hands, a varying group of offers was used, ranging from extremely poor in terms of the fair bet to extremely good. An arbitrary group of different hands plus the different offers combined with them form a "series." For instance, a series might be hands 1, 2, 3, 4, and 7 of Table 3 with four different offers per hand, making a total of twenty combinations. Several series differing in composition and length were used, for reasons to be given below in the section on "Experimental Design." The order within a series was randomized before its initial presentation. Two to five repetitions of a single series make one session. The subjects' participation as well as the length of a series governed how many repetitions were carried through in one session. There were no "breaks" between series, and play went on continuously.

TABLE 4

LIST OF HANDS DISPLAYED AND OFFERS MADE IN UNCERTAINTY AND LEARNING SESSIONS
(Number in Parentheses Is Serial Order of Hand in Series)

	Hand						
	44321	66431	55221	22263	55562	22255	44441
Offers	(14) 7¢	(42) 10¢	(11) 16¢	(39) 43¢	(24) 72¢	(41) $1.42	(46) $8.00
	(6) 6¢	(32) 9¢	(21) 13¢	(44) 37¢	(48) 61¢	(12) 1.21	(16) 7.50
	(47) 5¢	(9) 7¢	(23) 12¢	(15) 31¢	(7) 56¢	(49) 1.11	(8) 7.00
	(25) 4¢	(19) 6¢	(30) 11¢	(2) 28¢	(13) 53¢	(20) 1.05	(29) 6.00
	(17) 3¢	(45) 5¢	(38) 10¢	(10) 25¢	(18) 50¢	(4) 1.00	(26) 5.50
	(36) 2¢	(27) 4¢	(43) 9¢	(22) 22¢	(35) 45¢	(28) 0.90	(3) 5.00
	(34) 1¢	(1) 3¢	(5) 7¢	(31) 19¢	(40) 39¢	(33) 0.79	(37) 4.00

Once a series was begun, it was completed within the session.

It would have been preferable to randomize each series each time it was used. Unfortunately, practical difficulties concerned with the experimenter's recording of the outcome of individual plays made continuing randomization difficult. The one initial order was kept for all occasions when a series was used, both for different sessions and for different groups of subjects. In Table 4 are all the hands used, all the offers used, and the order within the series used in the Uncertainty situation and the learning sessions.

TABLE 5

COMPARISON OF ACTUAL WINS WITH EXPECTED WINS
(Uncertainty Sessions)

Hand (And Fair Bet)	No. of Times Played	No. of Times Won	No. of Expected Wins	Per Cent Times Won	Per Cent Expected Wins
44321..... 2.5¢::5¢	255	173	170	67.8	66.67
66431..... 5¢::5¢	269	132	134	49.0	49.75
55221..... 10¢::5¢	279	88	93	31.5	33.22
22263..... 25¢::5¢	302	49	50	16.2	16.67
55562..... 51¢::5¢	261	17	23	6.5	8.95
22255..... $1.01::5¢	284	12	13	4.23	4.71
44441..... $5.07::5¢	339	3	3.3	0.88	0.98

G. EXPERIMENTAL DESIGN

A number of different experimental conditions was used within the framework of the game and procedure of play described. These conditions were structured (1) to teach the subject about the game and give him an opportunity to gain experience in the play, (2) to derive experimentally utility curves for each individual, and (3) to check experimentally predictions about the subjects' behavior based on the utility curve constructed from previous sessions.

The first sessions for all groups are classed as "Uncertainty" sessions because during these the subjects did not know and, questioning revealed, could not calculate the true probabilities or mathematically fair bet for any hand. The Uncertainty situation consisted of three sessions, and the series used (Table 4) contained forty-nine combinations—seven hands with seven offers each. The series used was the same for all three groups of subjects.

At the end of the three Uncertainty sessions, each group met to learn how to calculate the true odds for any hand. In addition, each subject was given a sheet with the true odds for all hands used (like Table 3) and *was required to keep this sheet in front of him in all future sessions.* At this instruction meeting the subjects were also shown copies of Table 5—giv-

ing the number of times each hand had been played during the previous sessions, the number and percentage of wins expected if the information of Table 3 were correct. Subjects were given an opportunity to discuss results and to ask the experimenter about probability problems. The instruction may have been convincing, for, when questioned at the end of the experiment, all subjects said that they accepted as true the information given them in the instruction session.

At this stage the subjects had before them the true odds for each hand; so the next group of sessions was called the "Known-risk" situation. For each group the series used in the first few sessions (the learning sessions) of the Known-risk situation was exactly the same as the series used for that group in the Uncertainty situation. Following the instruction meeting, four learning sessions were used for each of the two student groups, and the National Guardsmen had three sessions. It should be emphasized that the *hands* used were not changed throughout the experiment, so the odds sheets continued to be pertinent.

The data obtained from the learning sessions were analyzed before proceeding further with the Known-risk situation. The purpose of the analysis was to ascertain how extant offers had to be changed to produce the kind of information desired by the experimenters. To derive the utility curve of a subject, it is necessary that for each hand the offers range from some so low that the subject will refuse to play most or all of the time to some so high that he will play most or all of the time. Since the utility curves—and, consequently, what is high and what is low—are expected to vary from subject to subject, a wide range of offers is needed for group coverage. This range can be determined only from the actual play of the subjects. (For some subjects, the amount of money required to induce play against a hand may be so large that a project such as this one cannot afford the information. The only information to be gained in such cases is that the subject's "indifference point"—50 per cent acceptance and 50 per cent rejection—for these hands is beyond a certain amount.)

After the analysis the offers for the remaining sessions of the Known-risk situation were adjusted to try to maximize the amount of information to be obtained from each group. The series used then varied from group to group. The data secured from the postlearning sessions were used to derive a utility curve for each subject.

An important part of the work is to obtain an independent check on the use of the utility curve to predict other behavior. To check the information contained in the individual utility curves, the Known-risk situation was modified by the introduction of a more complex bet. This modified procedure was termed the "Doublet" situation. Combination offers using two of the previous hands were devised. The stimulus cards carried a notation like this:

22263 :: 20¢
66431 :: 3¢

This meant: "You have the opportunity of betting or not betting 5 cents against this double offer: if you beat 22263 you will receive 20 cents; if you do not beat 22263 but do beat 66431, you will receive 3 cents; if you do not beat either, you will lose the 5 cents you must risk to play. You will roll the dice only once." During the Doublet sessions the subject had in front of him a new sheet giving the fair bet for each double combination presented. These data were for use in check-

ing behavioral predictions growing out of the data from which the utility curve was constructed. This point will be expanded later.

Among the criticisms offered of attempts to measure utility in risk situations is that participation in the gambling situation may have utility. To obtain some information on this point an "Investment" session was held. In the Investment situation the subject himself did not play but wrote out a set of instructions to be followed by an agent. This agent in the absence of the subject would rigidly follow the subject's instructions in playing with a dollar of the subject's money. The instructions given to subjects for these investment sessions were:

> One dollar ($1.00) will be taken from your reserve fund and given to some person whom you will designate. This person will use instructions which you are to write out for him below.
>
> Below is a list of the hands against which you've played. Opposite each hand you are to check or to write in the lowest offer for which your representative may wager five cents (5¢) against beating the hand with one roll of the dice. He will play against any hand only when the amount you indicate, *or more*, is offered. After he plays, *the total proceeds* of his playing will be given to you. You may not be present, however, when he is playing.

As a matter of fact, the agent never did play for the subjects in the experiment proper, although this had been done in the pilot study. Although this omission seemed sensible at the time, in retrospect the authors view it as a procedural error.

For other purposes, not the subject of this paper (although a brief report will be included later) a "Paired-comparison" situation was used. Two bets were presented on a single card, and the subject *had* to choose to play against *one* of the offers.

In Table 6 is a summary of the experimental design, listing the kind and number of sessions. For each session the number of rounds (one hand with one offer) and the average earnings for the three groups of subjects are listed.

IV. ANALYSIS OF RESULTS

A. UTILITY CURVES

As the first step in the analysis of the data, it was necessary to find the "indifference offer" to each subject on every hand. For each hand a range of offers had been made (see Table 7). The proportion of times the subject elected to play each offer was calculated (see example for subject B-I in Table 8), and these points were plotted on ordinary arithmetic graph paper with vertical axis as per cent participation and horizontal axis as amount of offer in cents (see Fig. 2). A freehand curve or a broken-line curve was then fitted to the points.[13] The abscissa value of the point where this curve crossed the 50 per cent participation line gave in cents the subject's indifference offer for that hand. In other words, for that hand this calculation yielded an interpolated offer which the subject would be equally likely to accept or reject if given the opportunity.

Since the probability p of winning is fixed for a given hand, the utility of the indifference offer is just $(1-p)/p$, as described in Section II. For example, the data of Figure 2 yield an indifference offer of 10.6 cents. Hand 55221 has a probability of 0.332 of being beaten. Therefore, the utility is about 2 utiles, and the utili-

[13] Various procedures were tried for smoothing the points: plotting on probability paper and on arithmetic paper, drawing freehand curves and broken-line segments. Fortunately, no dilemma arose, because the results were much the same no matter what techniques of plotting and fitting were used.

ty curve of subject B-I will pass through the point (10.6, 2).

The number of points on the utility curve which can be determined in this fashion equals the number of different hands which were used in the experiment. In the present experiment the value in cents of $\frac{1}{2}$, 1, 2, 5, 10, 20, and 101 utiles

TABLE 6

SUMMARY OF EXPERIMENTAL SESSIONS

Type of Session in Chronological Order	Group A Harvard Undergrads.		Group B Harvard Undergrads.		Group C National Guardsmen	
	No. of Rounds*	Av. Earnings per Subject	No. of Rounds	Av. Earnings per Subject	No. of Rounds	Av. Earnings per Subject
Uncertainty	49	$0.96	49	$0.45	49	$1.02
Uncertainty	98	1.05	98	0.87	98	0.47
Uncertainty	98	3.01	98	1.48	98	1.44
Paired comparison	42	0.90	42	0.90	42	0.53
Investment						
Instruction	Subjects told odds and instructed in calculating these odds					
Known risk	49	0.78	49	1.07	49	0.73
Known risk	98	0.87	98	1.78	98	1.34
Known risk	98	1.44	98	1.31	98	2.49
Known risk	98	2.66	98	1.72	Omitted	
Known risk	*94*	2.98	Omitted		*111*	1.54
Known risk	*92*	1.68	*99*	1.65	*129*	3.41
Known risk	*92*	2.11	*140*	1.41	*129*	0.26
Known risk	*138*	1.02	*140*	1.33	*132*	0.03
Known risk	*92*	2.34	*102*	0.89	*88*	0.47
Investment						
Known risk	Omitted		Omitted		*132*	1.47
Known risk	Omitted		Omitted		*132*	1.50
Doublet	100	0.91	100	2.11	90	0.45
Doublet	100	1.14	100	2.74	90	1.25
Doublet	110	1.21	100	1.60	110	1.07
Doublet	Omitted		Omitted		50	0.25
Paired comparison	60	1.04	60	1.05	60	0.50
Doublet	100	2.75	100	2.32	100	1.31

* Italicized rounds are ones used for constructing utility curves.

TABLE 7

THE ENTRIES SHOW FOR EACH GROUP AND EACH HAND THE RANGE OF MONETARY OFFERS MADE IN THE COURSE OF THE EXPERIMENT

(The Subject Puts Up 5 Cents)

Hand	Group A	Group B	Group C
44321	1¢– 7¢	1¢– 7¢	1¢– 7¢
66431	3¢– 10¢	3¢– 10¢	3¢– 10¢
55221	7¢– 16¢	5¢– 16¢	3¢– 16¢
22263	19¢– 43¢	13¢– 43¢	10¢– 43¢
55562	39¢– 72¢	28¢– 72¢	11¢– 72¢
22255	79¢–$1.42	79¢–$1.63	23¢–$1.42
44441	$4.00–$8.00	$4.00–$10.00	25¢–$8.00

TABLE 8

THIS TABLE SHOWS FOR SUBJECT B-I AND HAND 55221 THE BASIC INFORMATION REQUIRED TO ESTIMATE HIS INDIFFERENCE POINT*

Offer (Cents)	No. of Times Presented	No. of Times Played	Per Cent (Times Played/ Times Presented)
16	14	14	100
12	14	13	92.9
11	14	9	64.3
10	14	4	28.6
9	14	1	7.1
7	14	0	0
5	11	0	0

* Here only data from the italicized Known-risk sessions indicated in Table 6 are used.

was determined (these are rounded values). These, and the arbitrarily defined points [$U(0¢) = 0$ utiles and $U(-5¢) = -1$ utiles] can be connected by straight-line segments to form the utility curve of a subject. In Figure 3, illustrations of the utility curves are given for a few subjects. For reasons of scale we have shown values for only a few different utile positions. Logarithmic scales would be

ford to make the offers necessary to get the subjects to choose to play (if such offers existed).

There was nothing in the experimental procedure which coerced any subject to play at any time. It was possible for a subject to take his dollar at the beginning of a session and not play, thus assuring himself of $1.00. It is interesting that this never happened.

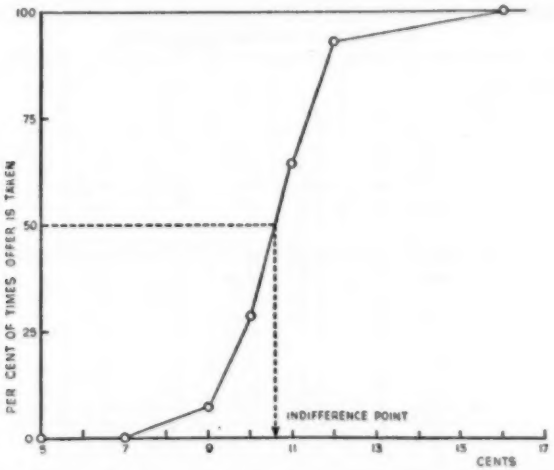

FIG. 2.—In this graph the data of Table 8 for subject B-I, hand 55221, are plotted to show how the indifference point is actually obtained.

somewhat misleading because some interest attaches to the curvature.

It was not possible to secure utility curves as complete as those in Figure 3 for all subjects. The behavior of one subject in the pilot study was so erratic that no utility curve at all could be derived for him. For two student subjects in the experiment it was possible to derive only a short section of the curve. Their indifference points for the high hands (i.e., those in which the probability of winning was small and which gave the values for 10, 20, and 101 utiles) were so high that the experimenters felt they could not af-

One subject showed markedly superstitious behavior toward one hand. He seldom played against it for any of the offers made, even though he would accept the same, or even smaller, offers against a hand which was less likely to be beaten. When asked about this after the project was completed, the subject said that he had been aware of his behavior but that he simply felt that the particular hand was unlucky for him and that he "just didn't like it."

In Table 9 are the indifference offers corresponding to each utility. When these are graphed, a rough utility curve

is obtained. Figure 3 shows such utility curves plotted for a few subjects. The straight line on each plot represents the "fair" curve. Casual investigation of Table 9 will show that the students (Groups A and B) tended to be "conservative," while the National Guardsmen were "extravagant." If support is desired for classical diminishing-returns curves, the student groups supply it. Support for the notion of increasing utility of money is given by the Guardsmen. It must be remembered, however, that this curve covers only ranges from −5 cents to about $5.50 for the students and only up to about $1.00 for the Guardsmen. It should be remembered, further, that all subjects had had instruction in the fair odds and in their computation and had before them at all times sheets giving them the fair odds for each hand played.

It seems clear that student Groups A and B differed in their reactions to the high-valued, low-probability hands. Every B subject wants more money to play the 20 to 1 and 100 to 1 hands than any A subject. Since there was no known selective factor between these two groups, supposedly this systematic difference between groups is a result of running the subjects as groups rather than individuals (see Sec. VID).

TABLE 9

AMOUNT OF MONEY IN CENTS CORRESPONDING TO VALUES ON THE UTILE SCALE FOR ALL SUBJECTS*

Subjects	"Fair" Offer −5¢ Utiles −1	0 0	2.5 0.5	5 1	10 2	25 5	51 10	101 20	507 101
A-I	−5¢	0	4.8	8.2	12.4	24.8	55.0	108	580
A-II	−5¢	0	3.8	5.2	9.9	28.5	58.0	121	583
A-III	−5¢	0	3.7	6.1	11.9	29.5	60.3	118	570
A-IV	−5¢	0	4.2	5.8	11.7	29.3	64.5	119	560
A-V†	−5¢	0	2.5	4.5	9.5	19.0	53.0	103	545
B-I	−5¢	0	4.8	5.0	10.6	28.5	60.0	163‡	1,000‡
B-II	−5¢	0	3.8	5.0	10.1	22.0	72.0‡	163‡	1,000‡
B-III	−5¢	0	5.0	6.5	16.0	22.0	44.5	163‡	990
B-IV	−5¢	0	4.5	5.4	11.2	20.0	42.0	139	657
B-V	−5¢	0	4.2	10.0	14.4	20.0	36.5	134	590
C-I	−5¢	0	4.1	6.2	§	12.7	22.5	66	92
C-II	−5¢	0	3.4	3.9	6.5	10.8	12.4	35	85
C-III	−5¢	0	4.7	7.8	9.0	21.6	23.5	55	175
C-IV	−5¢	0	3.9	4.7	9.5	20.6	28.0	62	103
C-V	−5¢	0	4.0	7.3	9.0	11.4	14.2	36	90

* The italicized Known-risk sessions of Table 6 were used in calculating these points of the utility curves. Columns for −5¢ and 0¢ are assigned, not measured.

† Data for three of five sessions. Subject left experiment.

‡ This number is the highest offer made, and it was refused by this subject more than 50 per cent of the time. Thus it is known only that the utility expressed in monetary units is larger than the value given.

§ Subject refuses all offers up to and including 16 cents. See discussion of superstitious behavior in text (Sec. IVA).

B. INVESTMENT SESSIONS

In the Investment sessions the subjects wrote instructions for an agent to use in playing for them. The second such session was given immediately after the Known-risk sessions used to determine the utility curves (i.e., the numbers of Table 9). On the basis of the instructions to the agent, it is possible to obtain approximate utility curves which should be

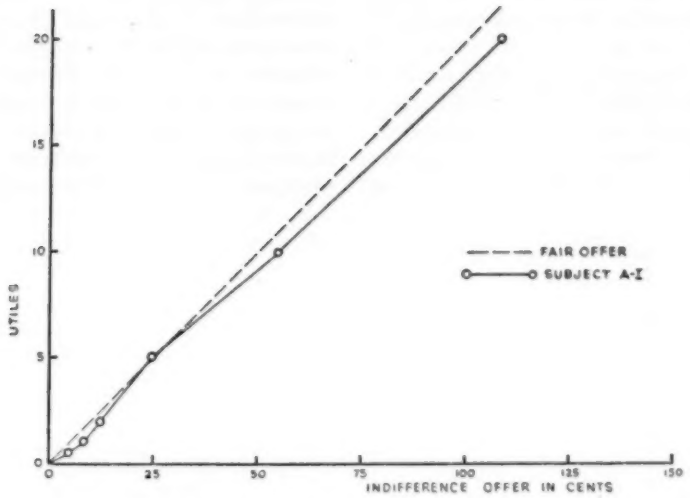

FIG. 3*a*.—Utility curve for subject A-I and straight line showing mathematically fair offer. Offer for 101 utiles not shown.

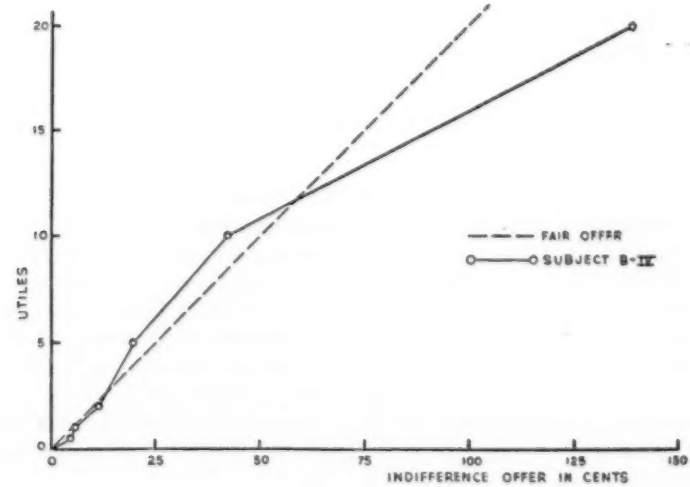

FIG. 3*b*.—Utility curve for subject B-IV and straight line showing mathematically fair offer. Offer for 101 utiles not shown.

experimentally independent of utility of participation (but not necessarily of utility of gambling, whatever this means). Comparison of Tables 9 and 10 shows that subjects in the student groups had about the same or a little higher indifference offers in the Investment session. But the Guardsmen group in every case had vastly higher indifference offers for the Investment sessions than for the Known-risk sessions. If one accepts the vertical difference between utility curves constructed from Known-risk sessions and Investment sessions as a measure of the value of participation, then the Guardsmen find participation more and more valuable as the sums involved increase. Compared with the Guardsmen, the students are relatively unaffected by participation.

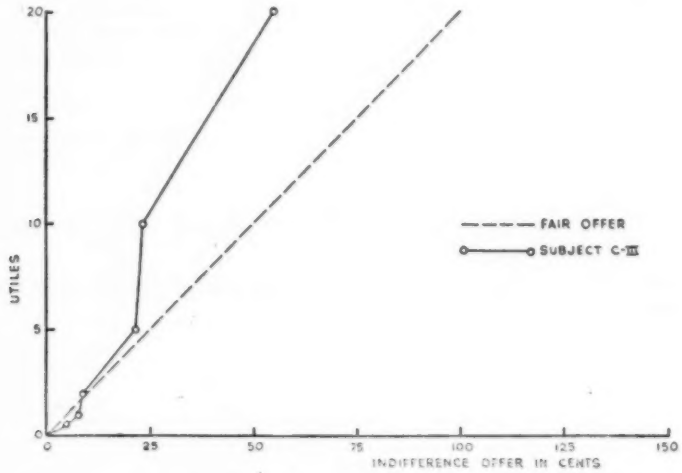

FIG. 3c.—Utility curve for subject C-III and straight line showing mathematically fair offer. Offer for 101 utiles not shown.

It was originally planned to have the second Investment session follow the last Known-risk session for all groups. When the Investment records of Group C were checked, however, the instructions for play written by these subjects appeared remarkably different from their actual behavior in the risk sessions. They seemed much less prone to accept offers on any hand which were less than the fair bet for that hand. The problem arose as to whether this new behavior was specific to an Investment session or whether it would persist if Group C were given further opportunities to play. To check this, two more Known-risk sessions were run with this group. Their behavior in these two sessions was approximately the same as that in the earlier Known-risk sessions, so the data of the italicized Known-risk sessions of Table 6 were used to construct utility curves for Group C, after all.

This paper-and-pencil method of testing has been criticized on the grounds that we are measuring what the subjects think is the socially accepted or proper thing to do or what they think we think

they should do, rather than giving information on how the subjects really want their broker to play. The critic points out that the paper and pencil may lend an air of formality and finality that is not present in the regular sessions, making the subjects more aware of being observed and therefore pushing them in the direction of conforming to a norm—say, the norm of the odds sheets. This may be the explanation of the Guardsmen's behavior. We are not in a position to check this view.

C. UTILITY-PARTICIPATION GRAPHS

After the results of the behavior are reduced to the forms shown in Table 9 or Figure 3, the question of predicting future behavior of the same individuals arises. According to the strict consistency

TABLE 10

AMOUNT OF MONEY IN CENTS CORRESPONDING TO VALUES ON THE UTILE SCALE FOR ALL SUBJECTS BASED ON INFORMATION OF SECOND INVESTMENT SESSION
(Columns for −5¢ and 0¢ Are Assigned, Not Measured)

Subjects	"Fair" Offer −5¢ Utiles −1	0 0	2.5 0.5	5 1	10 2	25 5	51 10	101 20	507 101
A-I	−5¢	0	6	9	16	31	60	121	600
A-II	−5¢	0	4	6	11	31	70	142	1,000
A-III	−5¢	0	3	6	12	28	61	121	600
A-IV	−5¢	0	4	7	12	30	61	125	600
A-V	Subject left experiment								
B-I	−5¢	0	5	5	10	35	72	125	1,000
B-II	−5¢	0	5	5	10	25	50	100	1,000
B-III	−5¢	0	5	7	20	25	50	142	1,000
B-IV	−5¢	0	5	5	10	25	50	121	700
B-V	−5¢	0	5	7	12	25	50	121	750
C-I	−5¢	0	5	8	16	21	40	90	500
C-II	−5¢	0	4	9	16	25	50	100	200
C-III	−5¢	0	5	10	10	30	60	100	400
C-IV	−5¢	0	5	9	12	25	50	121	600
C-V	−5¢	0	10	13	20	50	100	200	600

FURTHER INSTRUCTIONS TO AGENT FOR INVESTMENT SESSION

STUDENTS

A-I No instructions.
A-II Play till you drop to 75¢ then *stop!!*
A-III No further instructions.
A-IV Just stick to these; go above, but not below.
B-I No further instructions.
B-II Do not play against two highest hands unless doing very well.
B-III Play hands 1–5 inclusive at all times when odds are as indicated or better.
Play 6 & 7 only when total *EARNINGS* are in excess of $1.00.
B-IV Only play to 22255 and 55221 when you are ahead. Otherwise instructions are the same.
B-V Keep away from double pairs, full house, and four of a kind, except when very good odds and when you think you can win. If you get low, play only very good odds.

GUARDSMEN

C-I If you are ahead you may play the four 4's for as low as $3.00.
C-II Just play according to what I checked off.
C-III If player finds that he is winning, he shall go ahead and bet at his discretion.
C-IV If his winnings exceed $2.50 he may play any and every hand as he so desires, but if his amount should drop below $0.60, he should use discretion in regards to the odds and hands that come up.
C-V Just play as if it was your own money.

assumption, the only predictions that can be made are that offers (or objects or probability combinations of objects) whose expected utilities are positive will always be taken, and those whose expected utilities are negative will never be taken. As the discussion of Section II indicates and as the data show, such an assumption is invalidated by the experiment. Failing such an assumption, the difference point and certain offers for subjects B-I, B-II, and B-III that were not measured, as explained above. Thus for most hands and offers used it is possible to compute the expected utility of a bet for a particular subject. From this information and the data on participation (per cent of all opportunities that the hand is played at the offer), a graph can be made for an individual showing a

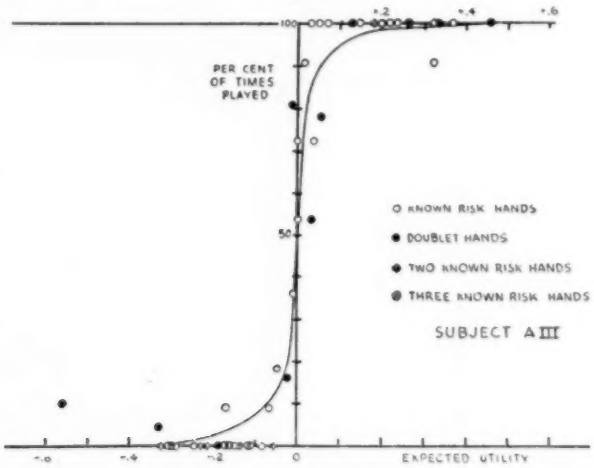

FIG. 4a.—Percentage times played plotted against expected utility for subject A-III. Curve is freehand fit to Known-risk points. The fit of this curve to the Doublet points shows roughly how good predictions of subsequent behavior are.

natural next-best prediction would be to say that offers whose expected utilities are positive will be taken more than 50 per cent of the time. Such a prediction is not so quantitative as could be desired, and it would seem to waste a good deal of the information contained in the data. One further possibility for unifying the data suggests itself. Since utility curves for individuals are now available, most offers used in the experiment involve amounts of money whose utility has been measured; exceptions are offers on the 44441 hand beyond the individual's in-

scatter of points for each expected utility, and a curve may be passed through these points. This was done by eye, with only points for Known-risk sessions plotted on the graphs (see Fig. 4, *a*, *b*, *c*, and *d*, showing four typical scatters). These four subjects were chosen to represent the student with least spread (A-III) and a medium-spread student (B-IV), the Guardsman with least spread (C-I), and a Guardsman with large spread (C-III). Such a curve is reminiscent of dosage-mortality curves in pharmaceutical work, where participation corresponds to mor-

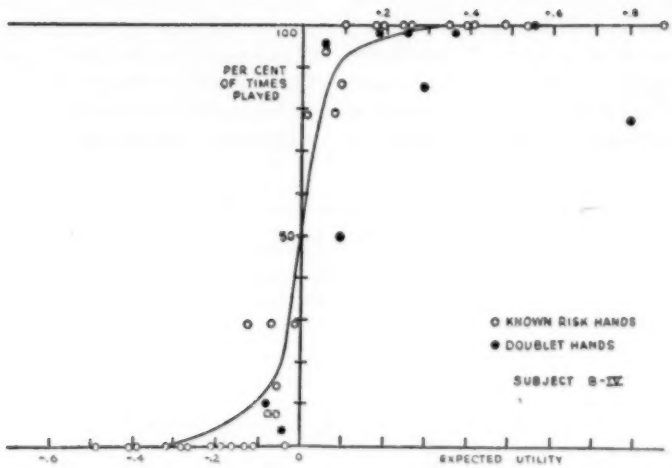

FIG. 4*b*.—Percentage times played plotted against expected utility for subject B-IV. Curve is freehand fit to Known-risk points. The fit of this curve to the Doublet points shows roughly how good predictions of subsequent behavior are.

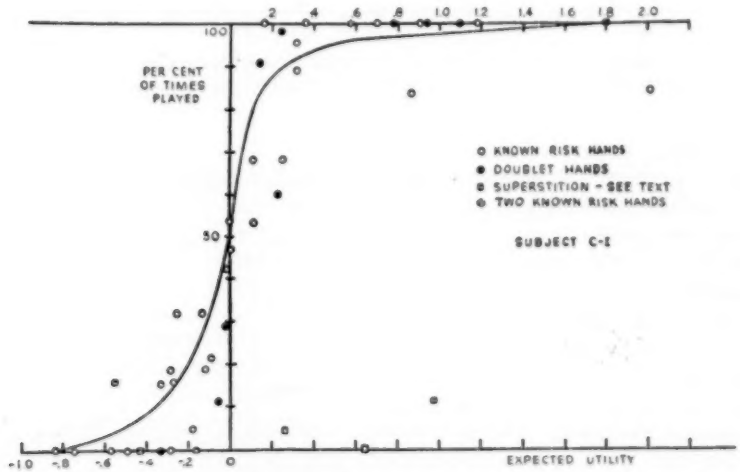

FIG. 4*c*.—Percentage times played plotted against expected utility for subject C-I. Curve is freehand fit to Known-risk points. The fit of this curve to the Doublet points shows roughly how good predictions of subsequent behavior are.

tality and expected utility to dosage, or, more generally, such a curve is like a classical sensitivity curve. These curves for individuals roughly resemble cumulative normal distributions. Although all the scatters pass across the zero expected cent participation point, subject by subject, as estimated from a rough graph of the subject's utility-participation curve. This quantity is roughly the standard deviation of the density function whose cumulative is represented by the scatter of

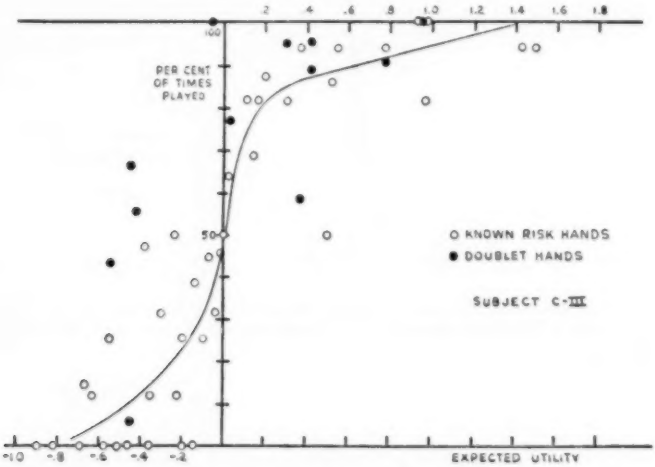

FIG. 4*d*.—Percentage times played plotted against expected utility for subject C-III. Curve is freehand fit to Known-risk points. The fit of this curve to the Doublet points shows roughly how good predictions of subsequent behavior are.

utiles at about 50 per cent participation, no credit for such a uniformity can be given because this finding was built into the expected utilities by the construction leading to the utility curves. However, it is pleasant to find a fairly uniform rise and drop of the points on the right and left of the origin, and it is still more gratifying to find that each subject has quite a decent-looking scatter of points. The general uniformity of such scatters suggests that, if one knew the expected utility of an offer for a subject, one could estimate approximately the proportion of times the offer would be taken.

In Table 11 is given half the estimated distance in expected utiles from the 16 per cent participation point to the 84 per

TABLE 11

INDEX OF DISCRIMINATION FOR SUBJECTS*

Subject	Group A	Group B	Group C
I	0.05	0.07	0.18
II	.07	.06	.23
III	.03	.06	.31
IV	.03	.06	.18
V	0.07	0.10	0.36
Average	0.05	0.07	0.25

* This is half the distance in expected utiles between estimated 16 per cent and 84 per cent participation from utility-participation curves.

points. Or, to provide a more palatable description, this index is a measure of the discrimination used by the subject—the smaller this index, the more nearly the subject approaches the ideal of the

man who always knows which of A and B he prefers or whether he is indifferent between them. A rough-and-ready interpretation would regard one utile as equal to 5 cents in the neighborhood of zero expected utilities. Furthermore, since the discrimination index is roughly a standard deviation, four standard deviations might be taken as the band width of the general indifference region or, better, "inconsistency zone" of an individual. Within this range the subject may regularly both take and refuse the same offer on different presentations, but to the right of this zone he will almost invariably accept the wager, while to the left he will almost invariably decline. Thus for student subjects the "inconsistency zone" in the neighborhood of zero is roughly 4×0.06 utiles $\times$ 5¢/utile = 1.2¢ long, while that for the Guardsmen is about 4×0.25 utiles $\times$ 5¢/utile = 5¢ long. To put it another way, the students have an eye for differences little larger than a penny when small amounts are involved, while the Guardsmen react about as frequently to differences amounting to a nickel.

D. THE DOUBLET SITUATION

To see whether more complicated offers than those used in constructing the utility curve would be handled by the subjects in a way that could be predicted from their expected utilities, the Doublet situation was used. Since there were two hands, there were three outcomes: (1) beat the high hand and receive A_1 cents; (2) beat the low hand but not the high one and receive A_2 cents; and (3) not beat either hand and lose 5 cents. So the expected utility of a Doublet hand is

$$E(U) = p_1 U(A_1¢) + (p_2 - p_1) U(A_2¢) + (1 - p_2) U(-5¢),$$

where

p_1 = probability of beating the higher hand,

p_2 = probability of beating the lower hand,

TABLE 12a

PREDICTED AND ACTUAL PARTICIPATION PERCENTAGES FOR DOUBLET HANDS AND NUMBER OF PARTICIPATION OPPORTUNITIES BY SUBJECT*

Doublets	Subject A-I			Subject A-II			Subject A-III			Subject A-IV		
	P	A	N	P	A	N	P	A	N	P	A	N
1	100	85	27	100	98	41	100	100	41	100	100	41
2	75	52	27	72	7	41	84	54	41	87	83	41
3	0	4	28	3	2	41	0	5	41	0	10	41
4	98	63	27	94	98	41	98	100	41	94	100	41
5	64	32	28	44	7	41	16	15	41	13	10	41
6	2	0	28	7	0	41	3	0	41	2	2	41
7†	99	95	21	100	100	31	99	100	31	99	87	31
8†	1	81	21	88	13	31	30	81	31	87	84	31
9	8	21	28	93	100	41	91	78	41	89	93	41
10	99	54	28	100	100	41	99	100	41	99	95	41
11‡	0	33	6	0	0	10	0	10	10	0	10	10
12‡	100	100	7	100	90	10	98	100	10	98	70	10

* Letters in columns have following meanings: P = predicted per cent participation; A = actual per cent participation; and N = number of observations upon which A is based.

† Used in all Doublet sessions but the first.

‡ Used only in first Doublet session.

and the utilities of A_1 and A_2 are read from the subject's previously constructed utility curve. In Figure 4 the solid circles are the results for the Doublet hands. In Tables 12*a*, *b*, *c*, and *d* are given predictions of per cent participation in Doublet hands by subjects (based on the utility-participation curves previously presented), along with the observed frequencies. Here the fact that hands with positive utility are usually played is *not* an artifact of the computation scheme, as it was in the original utility-participation graphs.

As was to be expected, the Doublet sessions caused considerable confusion,

TABLE 12*b*

PREDICTED AND ACTUAL PARTICIPATION PERCENTAGES FOR DOUBLET HANDS AND NUMBER OF PARTICIPATION OPPORTUNITIES BY SUBJECT*

Doublets	Subject B-I			Subject B-II			Subject B-III			Subject B-IV			Subject B-V		
	P	*A*	*N*	*P*	*A*	*N*	*P*	*A*	*N*	*P*	*A*	*N*	*P*	*A*	*N*
1	†	98	40	†	10	40	95	100	40	99	85	40	93	83	40
2	26	75	40	†	8	40	87	98	40	92	50	40	88	40	40
3	6	43	40	†	5	40	14	3	40	17	3	40	50	3	40
4	90	35	40	†	83	40	96	98	40	100	98	40	94	100	40
5	41	10	40	†	80	40	94	98	40	98	98	40	92	98	40
6	1	0	40	0	10	40	3	5	40	12	10	40	30	85	40
7	100	100	40	100	100	40	97	100	40	100	100	40	89	100	40
8	91	100	40	94	100	40	7	28	40	81	95	40	23	78	40
9	92	98	40	96	100	40	75	33	40	97	98	40	15	33	40
10	95	100	40	†	100	40	100	100	40	100	78	40	100	98	40

* For meanings of letters in columns see note * to Table 12*a*.

† Would require extrapolation of utility curve to determine the expected values of the hand.

TABLE 12*c*

PREDICTED AND ACTUAL PARTICIPATION PERCENTAGES FOR DOUBLET HANDS AND NUMBER OF PARTICIPATION OPPORTUNITIES BY SUBJECT*

Doublets	Subject C-I			Subject C-II			Subject C-III			Subject C-IV			Subject C-V		
	P	*A*	*N*	*P*	*A*	*N*	*P*	*A*	*N*	*P*	*A*	*N*	*P*	*A*	*N*
1	†	100	35	†	98	42	87	95	44	96	94	33	†	97	39
2	†	60	35	†	19	42	11	5	43	29	36	33	†	23	39
3	84	91	35	100	83	42	85	95	43	84	91	33	91	72	39
4	35	11	35	89	40	43	54	77	44	43	61	33	82	54	39
5	12	0	35	55	5	41	7	43	42	3	10	32	22	8	39
6	90	97	35	95	100	43	86	59	44	82	85	33	87	100	39
7	42	29	35	88	78	41	13	56	43	4	66	32	83	85	39
8	98	100	35	100	100	42	95	100	43	91	100	33	92	100	39
9‡	100	100	35	100	100	35	93	91	35	100	100	23	97	97	30
10‡	97	100	35	98	100	35	87	89	35	89	100	24	86	100	30
11§	.....	.....	‖	99	100	6	36	100	8	43	89	9	90	100	9
12§	.....	.....	‖	14	100	7	11	67	9	3	56	9	13	100	9

* For meanings of letters in columns see note * to Table 12*a*.

† Would require extrapolation of utility curve to determine the expected values of the hand.

‡ Used in all Doublet sessions but the first one.

§ Used in the first Doublet session only.

‖ Absent.

which is reflected in a larger inconsistency zone. On the other hand, the predictions from the Known-risk to the Doublet sessions seem rather satisfactory to the authors. It should perhaps be mentioned that most of the small percentage differences in Table 12*d* come from the extremes of the scale.

TABLE 12*d*

SUMMARY OF TABLES 12*a*, *b*, AND *c* FOR DOUBLET HANDS WITH MORE THAN 20 OBSERVATIONS

Table	Frequency Tabulation of Difference between Predicted and Observed Results for Doublet Hands in Percentages					
	0–9	10–19	20–29	30–39	40 and Over	Total
12*a*...	25	5	1	4	5	40
12*b*...	22	9	2	2	8	43
12*c*...	24	11	4	1	4	44

E. THE PAIRED-COMPARISON SITUATION

In the Paired-comparison situation the subjects were forced to choose between two hands. Each of these hands, of course, had its own expected utility for the subject, based on his previously derived utility curve. The utility-curve method, then, might be compared with the money-value method in predicting which choice would be made by the subject. In other words, the utility-curve method would predict that the hand with the larger expected utility would be preferred, while the money-value method would predict that hands with the higher expected money value would be preferred. In Table 13 these results are compared. Here a case means one pair of offers presented to one subject. Of course, each pair was presented several times, and the subject's over-all preference is being compared with the prediction. The letters "R" and "W" stand for "right" and "wrong" predictions. The "?" means that expected utility is identical for the two hands as nearly as we were able to tell, while "Imp." means "impossible" to tell because the expectancy in utiles could not be calculated for one of the hands. This calculation difficulty did not destroy the usefulness of these hands for their original purpose, which is not reported here. As a matter of fact, the present comparison is an afterthought and was not originally envisaged in the experimental design. In groups A and C the money-value and utility predictions are about equally accurate. But in Group B, of the 23 cases of disagreement, the utility computation is correct 21 times.

Two further computations of interest show the percentages correct as a function of the difference between the offers in a pair measured in expected utiles and in expected money value. The lines of the table roughly correspond. Thus the 54 per cent correct for the utility method corresponds approximately to the 42 per cent correct for the money-value method. In Table 14 the same 12 cases are missing from the summary of the utility method that are shown as "?" or "Imp." in Table 13. If the reader feels that any injustice has been done, he may split the 8 "?" entries 4 and 4 in the 0.01 to 0.15 line of the utility table. He will then find only 53 per cent correct instead of 54 per cent for the utility method. This leaves only 4 cases unaccounted for.

This comparison suggests that the utility method can be used to compare two offers and that it is somewhat more successful as a predictor than the expected money-value method.

F. THE DECISION TIMES

An extended discussion of the times taken by subjects to decide to play or not to play will be given elsewhere. It is of in-

terest, however, to know that these decision times have some relation to the notion of utility. In particular, as the expected utility goes from negative to positive, the average time taken by a subject to decide *to play* decreases. Similarly, as the expected utility goes from negative to positive, the average time taken by a subject to decide *not to play* increases. If straight lines are plotted to represent the decision times as a function of expected utility, the line representing "play" will have a negative slope, and the line representing "not play" will have a positive slope. Computations reveal that the utility co-ordinate of the intersection of these lines, which represent the relation between time measurements and expected utility, comes rather close to the zero point of the expected utility scale. Such computations supply an independent check on the reasonableness of the utility measurements and, moreover, give an additional way of thinking about the concept of indifference. This phenomenon is not an artifact of the experimental design but is a bona fide experimental finding. It could just as well have happened that the "play"-"not play" lines would not have intersected at all, or at least that they would not have intersected in any reasonable neighborhood of the zero point of expected utility. It is true, of course, that common sense suggests that the slopes of the lines would be as they are.

TABLE 13

COMPARISON OF PREDICTIONS OF PREFERENCES MADE BY THE EXPECTED UTILITY AND THE EXPECTED MONEY-VALUE METHODS IN THE PAIRED-COMPARISON SITUATION*

Utility Value	Money Value			Utility Value	Money Value		
	R	W	Total		R	W	Total
	Group A				Group B		
R	15	7	22	R	10	21	31
W	5	10	15	W	2	9	11
?	1	2	3	?	3	1	4
Imp.	0	0	0	Imp.	1	3	4
Total	21	19	40	Total	16	34	50
	Group C				All Groups Combined		
R	23	9	32	R	48	37	85
W	9	8	17	W	16	27	43
?	1	0	1	?	5	3	8
Imp.	0	0	0	Imp.	1	3	4
Total	33	17	50	Total	70	70	140

* The letters "R" and "W" stand for "right" and "wrong" predictions, "?" for equality of the bands, and "Imp." for impossibility of prediction. A case represents the preference displayed by one subject for one pair of offers.

V. PSYCHOLOGICAL PROBABILITY

Preston and Baratta[14] reported in 1948 an experiment designed to study the no-

[14] *Op. cit.*

tion of *psychological* probabilities in relation to mathematical probabilities through the medium of a game. Participants used play money to bid for gambles. A gamble is a prize with a certain probability of winning attached. Example: prize of $500 play money with probability of winning 0.75. The mathematical expected value is $375, and the average successful bid was $305. From such information Preston and Baratta estimated what psychological probability was needed to correspond to the mathematical probability of 0.75. In the example given, it would be about 305/500 = 0.61. On the basis of such data, the authors graph an average relation between mathematical and psychological probability. Using the data of the present article, similar psychological probabilities can be derived for comparison with Preston and Baratta's results. This is done in Table 15, where the Preston and Baratta numbers are read from their Figure 1, and student and Guardsmen results are medians from the present study.

There certainly is general agreement between the Preston and Baratta data and the numbers of the present study. On the other hand, one of their findings is that the "indifference point on the scale of probabilities" (psychological probability *equals* mathematical probability) is in the neighborhood of 0.20. For students, no such point was discovered, but for Guardsmen there is such a point in the neighborhood of $p = 0.5$. An explanation that immediately leaps to mind is that Preston and Baratta have mixed subjects, naïve and sophisticated in knowledge of probability. This happy explanation falls to the ground when it is discovered that knowledge of probability is one of the variables which those au-

TABLE 14

NUMBER AND PERCENTAGE OF CASES* CORRECTLY PREDICTED AS A FUNCTION OF DIFFERENCE IN EXPECTED UTILITY AND OF DIFFERENCE IN EXPECTED MONEY VALUE

Prediction by Expected Utility Value				Prediction by Expected Money Value			
Difference in Expectancies in Utiles of the Two Offers	All Groups Combined			Difference in Actuarial Values (in Cents) of Two Offers	All Groups Combined		
	No. Right	No. Wrong	Per Cent Right		No. Right	No. Wrong	Per Cent Right
0.01–0.15	37	32	54	$0.02–$0.50	34	46	42
0.16–0.30	21	7	75	0.55– 1.55	17	13	57
0.31–0.45	13	3	81	1.80– 2.20	9	6	60
0.46 and above	14	1	93	2.50 and above	10	5	67

* A "case" represents the preference displayed by one subject for one pair of offers.

TABLE 15

COMPARISON OF TRUE AND PSYCHOLOGICAL PROBABILITIES FROM PRESTON AND BARATTA AND FOR THE STUDENT AND GUARDSMEN GROUPS OF THE PRESENT EXPERIMENT

Approximate True Probability	P & B*	Students $N = 10$	Guardsmen $N = 5$
0.667	0.55	0.54	0.56
.498	.42	.47	.50
.332	.26	.30	.36
.167	.15	.16	.28
.090	.14	.081	.18
.047	.12	.038	.083
0.010	0.07	0.0085	0.052

* Preston and Baratta.

thors controlled in their experiment. Thus no ready explanation of the discrepancy between the results is available. It is also interesting to note that Griffith,[15] through a study of horse-racing odds (after correction for track take), found a corresponding indifference point at about $p = 0.16$ (although he is unwilling to go along with Preston and Baratta's complete interpretation). The results of the present study seem not to agree too well with either Preston and Baratta or Griffith in this respect.

In the actual play of the game, participants bid many times, and the winning bidder drew essentially from a table of random numbers to decide whether he won the particular gamble or not. The funds of subjects depleted or piled up as the game progressed. At the end the participant with the most play money received a prize. This would suggest that the auctioning system might not be independent from hand to hand but that bidding behavior should depend drastically on an over-all strategy designed to get the single prize, since it does not matter by how much one loses if he loses. Such a strategy might be much in evidence toward the end of the game, when it became clear that the losing members must gamble or lose the prize. Preston and Baratta kindly made available to us information about the sequences in which their gambles were presented. Our analysis of their results by sequences did *not* reveal to us any evidence of differential bidding for gambles at the beginning and end of the game; therefore, the strategy explanation does not seem adequate. There are further conjectures available as to why such an effect would *not* be found, but it seems best to get off the conjecture train here.

[15] R. M. Griffith, "Odds Adjustments by American Horse-Race Bettors," *American Journal of Psychology*, LXII (1949), 290–94.

The present authors find some difficulties in the notion of psychological probabilities that need to be cleared up. First, is the total psychological probability in a situation to be fixed at unity, and does additivity hold? If so, just how is this done? To give an illustration, suppose the mathematical probability of A happening is 0.5, and that of not-A happening is also 0.5; then, using Preston and Baratta's curves, the psychological probability of A is 0.42. The same psychological probability value would hold for not-A, and, assuming additivity, the total probability would be 0.84, which is less than unity. The failure of the total probability to be unity seems unsatisfactory.

By using a many-choice situation and small mathematical probabilities, a situation could be constructed in which the total psychological probability would be greater than unity on the basis of Table 15, because for small mathematical probabilities Preston and Baratta find that the psychological probabilities exceed the mathematical, as we do for the Guardsmen. In a two-choice (A, not-A) situation, these troubles could be partly avoided. It could be argued that an individual orients himself psychologically to a particular outcome—say, A—and splits the total probability into $P_S(A)$ and $1 - P_S(A)$, where $P_S(A)$ means psychological probability of A. Such an approach might handle two-choice situations, but it is difficult to see how probabilities derived from two-choice situations could be used to predict in the Doublet situations (where three probabilities must be known).[16] This kind of difficulty makes

[16] Ramsey (*op. cit.*) seems to be able to avoid the problem of setting up a correspondence between empirical (or mathematical) probabilities and psychological probabilities by introducing the concept of "degree of belief" in a proposition p. If several propositions have equal empirical probabilities, their degrees of belief apparently need not be the same in Ramsey's system.

it impossible to compare some results of the present study with theirs.

Such criticisms do not affect the importance of Preston and Baratta's experiment, but they do call for some further discussion of one of their interpretations.

VI. DISCUSSION

A. GENERAL RESULTS

From the material of Section IV, we conclude that for simple wagers it is possible to construct utility curves experimentally. There are considerable individual differences in the utility curves, as indicated by the results of Table 9, but the great difference is between the student groups and the Guardsmen group. The former, by and large, behave in accordance with classical diminishing-returns notions, while the latter do not, at least over the range of values measured. In so far as both increasing and diminishing returns have been observed, there is some experimental support for Friedman and Savage's arguments about inflection points in the utility curve, although it is true that such inflection points have not been very reliably observed within single subjects.

The utility curves were used to predict future behavior on more complicated wagers. These predictions are not so good as might be hoped, but their general direction is correct.

The notion that subjects would consistently prefer A to B or consistently be indifferent to A or B was not supported.

B. SOME CRITICISMS: EXPERIENCE

One of the uncontrolled factors in the experiment was the amount of experience a subject had in playing against each hand. This experience depended upon the number of times which a subject and the other members of the group with which he met had elected to play against each hand. The number of times a subject could see what happened when either he or other members of the group played against a particular hand could vary widely from group to group. The only hands on which very large differences did occur, however, were the two for which the probability of winning was least (44441 and 22255). For example, in all Known-risk sessions, Group B played against hand 22255 only 73 times, while Group A played against it 341 times, and Group C 520 times.

Professor John Tukey, of Princeton University, has suggested that this problem be handled by making every subject roll at every opportunity, whether or not he had elected to bet. The result, of course, would not count, either for or against him, if he had not elected to bet. The only disadvantage of this procedure is the amount of extra experimental time it would require. Both the greater amount of experience and the greater uniformity of experience which the subjects would get on each hand would be marked advantages. This suggested procedure might well be adopted in any further experimentation.

C. EFFECT OF THE AMOUNT OF MONEY IN FRONT OF SUBJECT UPON HIS DECISIONS

In evaluating results, one difficulty stems from the fact that the subject had differing amounts of money in front of him when he made decisions. In both the pilot study and the experiment a subject "rolled" immediately after making a decision to play. If he won, he was immediately given an amount of chips corresponding to the offer; and, if he lost, he had to give the experimenter a white chip worth 5 cents. Each time he played, then, the amount of money he had on hand changed before his next decision.

One possible criticism could be that the subject changes his utility curve with these changes in capital, so that each decision he makes depends upon the amount of money he has on hand at that particular moment. Such a criticism is not unreasonable—indeed, there are some data available from the pilot study to support this criticism. Furthermore, written instructions for the Investment session (Table 10) are in some cases contingent on the total amount of money on hand. An analysis of frequency of play broken down by amount of money on hand and by monetary offer is quite expensive to make. This analysis was carried out for some subjects in the pilot study but not for subjects in the experiment. The pilot-study subjects had utility curves similar to those of the student subjects in the experiment.

Some of the results of this analysis are shown in Table 16. Rather than use space for all the data, a systematic sample will be given. Subjects were arbitrarily numbered from 1 to 8, and the first two subjects displayed for the lowest hand, the second two for the next lowest, etc. The best information on this is obtained from an examination of the middle offers. Subjects P-I, P-VI, and P-VIII seem to show a clear trend of being more likely to accept an offer as their money on hand increases, as examination of Table 16 reveals. In this table notice that subject P-I, in playing against the even-money hand, played against offers from $5\frac{1}{2}$ to 6 cents, with frequencies depending on the amount before him. When he had 75 cents or less, he played 24 per cent of the 21 times he could have; with 76–175 cents he played 59 per cent of the 17 times he could have; and with more than 175 cents on hand he took all but one of the 14 offers.

One way to handle this problem would be to change the experimental procedure. Instead of having the subject roll immediately after each decision to determine the outcome, one could have him first make all decisions for the entire series of offers used in any session. After he had completed this part, he would then roll against those offers where he had decided to play. If he ran out of money at any point during this second part, he would, of course, be required to stop; but all his decisions could still be used for calculating his utility curve. This change in procedure would have at least two marked advantages. All decisions of the subject would be made while he had a constant amount of money in front of him—his original endowment for the session of $1.00. And his decisions would be uninfluenced by experience of success or failure during that session. On the other hand, by interposing a relatively long delay between the decision and knowledge of the outcome of that decision, this procedure might make the events seem more mechanical and less "realistic" to the subject, and so affect the results detrimentally. As a matter of fact, there is some evidence that the Guardsmen would be affected by such a procedure. In the Investment session their written decisions did not correspond to their behavior in the actual play of the game. This may be a much more serious factor than the influence of the amount of money on hand.

In the pilot study, offers involving half- and even quarter-cents were tried, as Table 16 suggests. It was thought that finer measurements could be obtained by using a finer scale. Such wagers turned out miserably. For example, a subject in possession of a chip worth half a cent would take another offer involving half a cent at odds much worse than those he

TABLE 16

The Relation between Amount of Money on Hand and Frequency of Participation for Selected Hands and Subjects Stratified by Offer*

Amount on Hand (Cents)	Offer in Cents			Total Per Cent	Offer in Cents			Total Per Cent
	Subject P-I				Subject P-II			
	1–5.25	5.5–6	6.5–10		1–4	5–6.5	7–10	
	Hand 66431							
5– 75	3, 36	24, 21	92, 13	30	0, 2	39, 28	71, 7	43
76–175	0, 28	59, 17	97, 29	51	5, 22	25, 16	84, 25	41
Over 175	0, 7	93, 14	100, 12	76	0, 6	31, 16	88, 8	40
	Hand 55221							
	Subject P-III				Subject P-IV			
	4–11	12	13–25		1–5.5	6–10		
5– 75	0, 25	44, 9	100, 24	48	0, 16	80, 20	.....	44
76–175	0, 12	25, 8	77, 22	45	0, 42	89, 46	.....	47
Over 175	0, 7	.....	100, 7	50	0, 2	100, 4	.....	67
	Hand 55526							
	Subject P-V				Subject P-VI			
	5–50	60–100			5–51	52–55	58–100	
5– 75	0, 11	89, 27	.....	63	0, 21	14, 14	95, 22	40
76–175	4, 27	86, 42	.....	54	0, 30	24, 29	100, 34	44
Over 175	0, 7	64, 11	.....	39	0, 7	58, 12	100, 28	75
	Hand 22255							
	Subject P-VII				Subject P-VIII			
	25–150	175–200			25–102	103–150	175–200	
5–75	0, 3	0, 2	.....	0	0, 43	23, 86	75, 8	19
76–175	2, 89	20, 30	.....	7	0, 11	63, 24	100, 3	47
Over 175	0, 3	33, 3	.....	17	0, 10	92, 24	100, 4	68

* The second entry gives the *number* of times the offer was made, while the first entry gives the *percentage* of times that the offer was taken.

had consistently refused. He was merely trying to get an integral number of cents. Thus the half-cent chip in front of the subject influenced his behavior unduly, from the point of view of the experimenters. In the experiment proper, fractions of cents were not used in any offers.

D. EFFECT OF GROUP PARTICIPATION

Ideally, each subject should have been put through the procedure individually, but this would have required an excessive amount of experimental time. Therefore, the compromise procedure of using small groups of five subjects was adopted (see last paragraph of Sec. IVA).

At the first session of each group the subjects were strongly warned not to make any comments about any other person's decisions. Throughout all the sessions this rule was rarely—in fact, almost never—violated. The strongest desires to violate this rule developed in the pilot-study group, which contained the one subject for whom no utility curve could be calculated. His unusual behavior apparently deeply offended the sensibilities of the other subjects, particularly since he did not always lose his money as a result of such behavior. In private interviews after the study was completed, the other subjects were sarcastic and contemptuous of his style of play. Yet they never commented upon his decisions during the actual sessions.

It might be thought that the subjects would form coalitions against the experimenters. If all subjects had consistently refused to accept a series of offers, the experimenters would have been forced to raise the offers in order to get information. A group coalition could have determined the limits of the offers made on any hand. That such coalitions did not occur was probably due to at least two reasons: first, the rule that the subjects were not to discuss the experiment either among themselves or with other persons and, second, the subjects seemingly never became aware of the reasons for changing offers to a hand. So far as they could see, this was independent of their own play. Within any one session no offers were changed at all by the experimenter, no matter what the subjects did. From session to session not all offers to a hand were ever changed, and often no changes were made.

E. THE UTILITY OF GAMBLING

The notion of utility of participation has already been discussed (see Tables 9 and 10), but there still is the criticism that gambling itself may have utility. The authors have no good experimental method of approaching this problem. Some attempts in the pilot study came to nothing. Nonetheless, this criticism has a tendency to be overworked—especially by people who have no notion how to measure such a utility. Mere exhilaration over winning is not enough to imply that gambling has utility; behavior must be changed before we shall be interested in this effect. Furthermore, there is no reason to assume that utility of gambling has a large effect, compared, say, to utility of participation, as discussed earlier. Indeed, the writers would prefer to defer discussion of this point until a way of testing arguments about it is provided.

F. TRIVIALITY OF SUMS INVOLVED

Some persons with whom the pilot study was discussed commented that the results might not be meaningful because the amounts of money which the subjects could obtain were trivial.

To meet this criticism, emphasis was placed on selecting for the experimental

study subjects who needed money. All student subjects were selected from rosters kept by the Student Employment Office. While participating in the experiment, these students had to give up other opportunities for part-time employment at which they could earn approximately a dollar for an hour's work. The National Guardsmen were selected as subjects to insure having a group with a less favored economic status than the student subjects.

In those Known-risk sessions from which the data for calculating the utility curves were obtained, the largest amount which a subject could win on a single round (i.e., the highest offer for beating hand 44441) represented a sizable fraction of his regular weekly income or expenditures. For the student subjects this offer amounted to approximately 30–50 per cent of their monthly room rentals and was generally either equal to, or more than, their average weekly earnings from other part-time work. For the National Guardsmen this offer amounted to 16–43 per cent of their monthly home rental and was equal to approximately 11–14 per cent of their weekly take-home pay from their regular jobs.

During the final interview all subjects were asked the uses they had made of their earnings from this study. Most students reported that they had used the earnings to pay for general living expenses, recreation, and books and other school supplies. One student, however, reported that he had used his earnings to repay some old debts incurred when he had owned a car. Most of the National Guardsmen also said that they had used their earnings for everyday living expenses. One, however, reported that he had saved his earnings to buy a layette for an expected baby. Another, who became unemployed during the course of the study, remarked that his winning a sizable amount in one session was very opportune, since it gave him the means of paying some overdue insurance premiums.

Although the amounts of money involved were small in comparison to those involved in many situations considered by economists, it seems apparent that both the possible and the actual earnings were not entirely trivial from the point of view of the subjects. On the other hand, from any long-run point of view the amounts were indeed trivial. No windfall from this experiment would be enough to buy a house or a new car or enough 6 per cent bonds to defray regularly the cost of the daily newspaper. Perhaps one can never meet this point squarely. As Ramsey says, "Since it is universally agreed that money has a diminishing marginal utility, if money bets are to be used, it is evident that they should be for as small stakes as possible. But then again the measurement is spoiled by introducing the new factor of reluctance to bother about trifles."[17]

VII. CONCLUSION

On the basis of the results of the experiment described in this paper, the authors conclude:

1. That it is feasible to measure utility experimentally.

2. That the notion that people behave in such a way as to maximize their expected utility is not unreasonable.

3. That on the basis of empirical curves it is possible to estimate future behavior in comparable but more complicated risk-taking situations.[18]

[17] *Op. cit.*, p. 176.

[18] Professor Armen Alchian, who discussed the present paper at the joint session of the American

4. That there is some support for the inflection-point analysis offered by Friedman and Savage, although this support is not wholly satisfactory—i.e., there is no contradiction, but the support is meager.

5. That subjects are not so consistent about preference and indifference as postulated by Von Neumann and Morgenstern but have a graded response, gradually increasing the frequency of risks taken as the value of the risk increases.

6. That this effect may be negligible in the practical situations which economists wish to consider.

7. That, in spite of extensive instruction and experience, some individuals continue to take wagers which lead to monetary losses in the long run.

Statistical Association, the American Economic Association, the Econometric Society, and the Institute of Mathematical Statistics, suggested that conclusions 1 and 2, in so far as they mean anything, mean the same thing as conclusion 3.

LABOR AND OUTPUT IN SOVIET CONCENTRATION CAMPS

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SECRECY has its reverse in exaggeration. The Soviets, not without decidedly good reasons, are making a secret of their concentration camps, and this gives free rein to exaggeration in the Western world. A publication of the American Federation of Labor contains the statement:

> Some have estimated the number in concentration camps as high as 20 million. Mr. Dallin uses a conservative figure of 8 to 12 million in 1947, after careful examination of all evidence. Mr. Kravchenko states that during the war "in official circles twenty million became the accepted estimate."[2]

B. I. Nikolaevsky's assumptions with reference to the camps of the Far East imply a very high total for the inmates in all camps:

> The importation of from 400,000 to 500,000 of slave colonists [to Magadan and other Dalstroi ports] each year appears credible. At any rate, the total number of prisoners brought into Kolyma region during the first ten years of Dalstroi's existence (i.e., 1932 through 1941) is in the millions.[3]

Figures of fifteen to twenty million in concentration camps are of course fantastic, implying, as they do, that nearly one-third, or more, of adult males are behind the bars. Specifically, Nikolaevsky's estimates would possibly be closer to the mark if a zero were omitted at the end of each of his figures. Even the eight million, believed very conservative, appears to be a great exaggeration upon closer analysis. In addition to consideration of the age and sex composition of the population, already alluded to, the geographical distribution of the camps calls for caution. The camps are normally located in entirely unpopulated or very thinly populated areas, with very limited economic activities.

The attempts of the Soviet government to conceal the existence of concentration camps made sense a few years ago. Especially in the favorable atmosphere of the U.S.S.R. as a war ally, many believed the Soviet denials or at least felt it wise to ignore the evil. It is different today, when dozens of former inmates are at hand to testify to the existence of the camps. Official recognition of the camps would not add much now to what everyone believes. But while the fact, unfortunate for the Soviets, that so many former inmates (an extremely minute percentage of their total number) finally succeeded in becoming free eliminates any doubt with reference to the existence of concentration camps, it does not help to solve the problem of the number of persons in the camps. The inmates are not in position to know their total number, and it is only human if they exaggerate, and exaggerate greatly.

[1] Thanks are due to George Denike, one of the best students of Soviet life, for valuable suggestions. I relied heavily on his familiarity with the publications of the former inmates. I am also pleased to mention that Mr. Denike is in general agreement with the analysis in this paper.

[2] *Labor Monthly Review*, IX, No. 5 (May, 1948), 6. The year in the citation, 1947, apparently pertains to the time of publication of the book cited in n. 3.

[3] Nikolaevsky in David J. Dallin and B. I. Nikolaevsky, *Forced Labor in Soviet Russia* (New Haven, 1947), p. 137. Dalstroi in the Kolyma region is the camp organization in the Soviet Far East, primarily engaged in gold-mining.

Fortunately, an official document which throws light on the number of persons in the camps has recently become available. It may be a disappointment to some believers in large figures that the number of inmates turns out to be much smaller than they have grown used to accepting. (I have always believed the large figures absurd.) But the document is an authentic testimony that at the end of 1940, when the U.S.S.R. was not at war, the Soviets expected to have millions in concentration camps in 1941, without, it should be noted, going to war and having prisoners of war to put into the camps. The millions of inmates to which the document testifies are not the exaggerations of naturally embittered former inmates, or similar fantasies of political writers with whom restraint is not a characteristic virtue, but real millions officially acknowledged. This document is commonly called the "1941 Plan," and hereinafter it is so cited.

THE 1941 PLAN

The official title of the document is *State Plan of Development of National Economy of the U.S.S.R. for 1941* (Supplement to the Order of SNK, U.S.S.R., and VKP[b], No. 127 of January 17, 1941). There are 377 supplements, all combined in one volume. Each supplement has a separate number, and each carries the warning "not to be disclosed." I have been unable to find the government order referred to in the supplements. Only a little information on the 1941 Plan was disclosed in due time. The principal source was the short report of Voznesenskii, then president of the Gosplan (State Planning Commission), to the Supreme Soviets. While the document here analyzed speaks freely of the economic activities of the organizations in charge of concentration camps, not a word of course is found on them in Voznesenskii's report or in other pronouncements on the 1941 Plan released without limitations as to circulation.

The authentic nature of this volume of 746 pages is beyond any doubt. I have studied carefully all published Soviet plans, and there is nothing in this document which would not fit perfectly into the line. There are, it is true, deviations from the released versions of the preceding plans. The so-called 1941 Plan does not have any text, nor does it contain the usual data for the base year, in this case 1940. But the absence of text is explained by the character of the document, consisting of supplements, while the absence of data for the base year must be due to the fact that the document, unlike the released plans, was not intended as an explanation but simply as an order for action by the persons concerned. Moreover, data for the base year are absent in the fourth Five-Year Plan released without any limitation as to circulation. So far as figures in the document are concerned, each one makes perfect sense in relation to all other evidence on the Soviet economy. There is also no apparent reason for anybody, and especially the Soviet government, to have put out a falsified document of this type and size.

A noteworthy feature of the document connected with concentration camps should be pointed out before turning to the main issue. Although each supplement is marked "not to be disclosed," there was a secret supplement or supplements which contained, along with data on armament production, such data as gold production and details of the output of nonferrous metals and synthetic rubber. Gold production in the eyes of the Soviets was a greater secret than particulars on concentration camps. These

were only a mild secret. Otherwise they would not have been discussed freely in a printed document which certainly had a relatively wide circulation. Moreover, the document not only speaks of the activities of the NKVD (People's Commissariat of Internal Affairs, now MVD, Ministry of Internal Affairs), which, although almost fully pertaining to concentration-camp labor, does not sound so revealing. The 1941 Plan unhesitatingly mentioned specifically such renowned hate-inspiring camp organizations and divisions as GULAG, UKhTIZhEMLAG, and DALSTROI.

Now as to these particulars on the concentration camps in the 1941 Plan. The document does not contain any data on the number of camps or of their inmates. Neither does it give any details on the economic activities of the NKVD, except for information that this commissariat (or its subdivisions) was expected to construct or produce "so-and-so much" in values or physical terms. The only exception, Supplements 262–63, is reproduced in Table 1.

The figures in Supplements 262–63 obviously could not have covered all economic operations of the NKVD in industry and of its GUZhOSDOR, as is apparent from the other data in the 1941 Plan. Furthermore, the average wages which were paid in the industrial enterprises of the NKVD for which Supplements 262–63 provide the data (about 5,000 rubles per year) substantially exceeded the average wages of all Soviet wage-earners, which was expected to average in 1941 not quite 4,300 rubles. There seems to be no other explanation than that the data of Supplements 262–63 pertained to employment of "free" labor (free so far as such exists in the U.S.S.R.) by the NKVD. The wording may even imply that this was all the free labor employed by it.[4] The rest of the NKVD industrial activities were apparently to be performed with concentration-camp inmates.

To interpret the data pertaining to concentration camps in the 1941 Plan, a few methodological remarks are necessary. But it must be emphasized that the computations made with the help of these interpretations necessarily involve a great deal of estimation, partly or even largely arbitrary, and that the conclusions at best will be within a rough range of approximation.

TABLE 1

LABOR PRODUCTIVITY,* NUMBER OF WAGE-EARNERS, AVERAGE WAGES, AND WAGE FUND FOR 1941 IN INDUSTRY AND GUZHOSDOR NKVD U.S.S.R.†

	Number of Workers (Thousands)	Average Wage (Rubles)	Wage Fund (Million Rubles)
Industrial enterprises:			
Total	16.82	.....	78.89
Workers	10.97	4,700	51.55
Technicians	1.50	8,442	12.67
Employees	1.06	4,700	4.97
Not specified (balance)	3.29	.....	9.70
GUZhOSDOR:‡			
Total§	60.68	.....	159.45
Workers	52.63	2,424	127.56
Technicians	2.26	6,421	14.51
Employees	1.89	4,203	7.94
Not specified (balance)	3.90	.....	9.38

* Output per worker in industry (rubles) = 21,302.

† 1941 Plan, p. 545, Supplements 262–63.

‡ GUZhOSDOR apparently stands for State Organization for Paved Roads.

§ Current and capital repairs of roads.

THE CONCEPTS INVOLVED

It is generally agreed that Soviet figures for totals of industrial production, construction, transportation, and so on are comprehensive, i.e., that they include

[4] Free labor employed by the NKVD in secret enterprises such as gold-mining may be additional.

also outputs which are not commonly specified, as, for example, output of armaments and gold. The outputs of the NKVD produced with camp labor must also be included in those totals.

It is my assumption that the last population census, i.e., that of 1939, also was comprehensive, the concentration-camp inmates probably appearing in the released version of the census under their occupations before coming to the camps. In the same way I assume that all other references to the population include the concentration-camp inmates. This does not, however, imply that the same is true of current labor-force statistics.

David Dallin, the recognized authority on Soviet concentration camps, apparently believes that all inmates are always included in the labor-force statistics. He bases his assumption on evidence received from former inmates.[5] But the assumption seems only partly correct. I believe that the concentration-camp inmates are included in the data on the labor force only in so far as the NKVD (MVD) hires out or otherwise hands over the inmates for work to enterprises of other commissariats (ministries) or organizations. In my opinion, the concentration-camp inmates are not included in labor-force statistics so far as they are employed in the NKVD's own enterprises.

The evidence for the assumption that those employed in such NKVD enterprises are not included is too involved to be presented here in detail. It will be shown later that the NKVD is a great factor in certain sectors of construction work. The 1941 Plan (p. 512) gave the total number of persons to be employed in construction in 1941 as only 3,085,000. It seems extremely unlikely that this figure included all concentration-camp inmates employed on construction projects of the NKVD. For the output per man in construction in that year would have been much too high, and the changes in the productivity of this labor from the preceding years much too large.

With reference to inmates hired out by the NKVD to other organizations, the following statement in the latest edition of a Russian textbook on industrial statistics seems to throw some light: "According to existing instructions on the forms [formulas] of accounting of industrial enterprises, persons used for work on the basis of contracts with state organizations (not in the way of hire) are included among the workers of the enterprises."[6] While there are categories of persons other than concentration-camp inmates who can be obtained for work otherwise than by direct hire, the secretive manner of Savinskii's statement indicates that he does not tell all that he knows. His presentation seems also to exclude definitely the use by the enterprises of any workers who are not included in the lists of the labor force.

While the 1941 Plan gives the numbers of wage-earners which were to be employed by the various enterprises, it does not state how many of these workers were to be concentration-camp inmates. I assume that the 1941 Plan provides no evidence concerning the concentration-camp inmates working in enterprises other than those of the NKVD and that an allowance for these inmates has to be made in the final computations.

ECONOMIC ACTIVITIES OF THE NKVD

Construction.—The most important activity of the NKVD is construction. The 1941 Plan does not provide suitable data on this item, but it contains data on

[5] Based on personal discussions with Mr. Dallin.

[6] D. Savinskii, *Course of. Industrial Statistics* (Moscow, 1949), pp. 148–49.

capital investments, of which construction is equivalent to 60 per cent or a little more, by value.[7]

The 1941 Plan (pp. 483–85) listed 37,650 million rubles' worth of capital investments, but the list did not include the capital investments of the Commissariats for Transportation, Defense, and Navy. The total was 48–49 billion rubles, according to the plan (p. 3); but even this had not yet included the small projects, called in the plan "below limit." N. A. Voznesenskii, then president of the Gosplan, in introducing the 1941 Plan, gave the total capital investments at 57 billion rubles, of which 9 billion rubles were to be decentralized investments.[8] The centralized capital investments which were to be carried through by the NKVD according to the 1941 Plan are shown in Table 2.

TABLE 2

CENTRALIZED CAPITAL INVESTMENTS TO BE PERFORMED BY THE NKVD IN 1941*

Agency	Million Rubles
NKVD, total	6,810
GULAG	2,675
GUZhOSDOR	550
GUZhDS	1,350
Not specified	2,235

* 1941 Plan, p. 484.

GULAG is the principal camp organization; the letters stand for State Organization of Camps. GUZhOSDOR has already been interpreted as the State Organization for Paved Roads, and GUZhDS stands for State Organization for Railway Construction.

If it is assumed that "below limit" of the 1941 Plan and "decentralized" of Voznesenskii were the same thing, and (in the absence of evidence to the contrary) that the NKVD had the same share in small projects as the other commissariats, the total capital investments to be accomplished by the NKVD were expected to be equivalent to around 8 billion rubles.

Fifty-seven billion rubles of capital investments implies roughly 35 billion rubles' worth of construction. The corresponding figure for the NKVD construction would be 4.9 billion rubles, but it is assumed to have been equivalent to 6 billion in view of the large share of road construction in the NKVD projects, road construction not requiring any installed machinery.

Since total construction was estimated at 35 billion rubles and that of the NKVD at 6 billion rubles, the planned construction activities of the NKVD constituted 17 per cent of the total.

The totals as computed above do not include repairs of capital equipment, a minor item. The NKVD is unlikely to have participated in this work to a large extent; on the other hand, the share of construction in the capital-investment activities of the NKVD may have been larger than the 75 per cent implied in the foregoing assumptions.

The 1941 Plan (p. 512) gave the total number of wage-earners engaged in construction as 3,085,000. As pointed out above, it is assumed that the inmates working on NKVD construction projects were not included in the above total. Hence the stated number of wage-earners has to be related to only 82.9 per cent of all construction. If the output per inmate were the same as that of free labor, about 670,000 inmates would have been necessary to perform the construction as-

[7] Capital investments are fixed investments of every kind, including those in military barracks, schools, hospitals, etc. On the share of construction in capital investments see my *Soviet Prices of Producers' Goods*, chap. ix (in press).

[8] *Pravda*, February 19, 1941.

signed in the plan to the NKVD. Productivity of labor is, however, much lower on NKVD projects than in other construction—to a certain extent because of inadequate machinery, to a large extent owing to lesser suitability of the workers to their particular jobs and to the labor's being forced. Hence, I arbitrarily raise the figure obtained above by 75 per cent, i.e., I estimate the number of concentration-camp inmates engaged in the NKVD construction projects as 1,172,000.[9]

Industry.—The 1941 Plan (pp. 9–10) gives the share of the NKVD in total industrial output at 1,969 million rubles, or 1.2 per cent of the total. The data are in so-called "unchangeable 1926–27 prices" which were not in proper balance for different commodities.[10] The figure of 1.2, as the percentage of the NKVD industrial output to total industrial output, may strike those who are used to astronomical figures in connection with Soviet concentration camps as far too small. The subsequent analysis shows, however, that the figure makes sense.

The total number of wage-earners scheduled in the 1941 Plan for industry was 11,092,000 (p. 512). An NKVD share of 1.2 per cent of the industrial output would imply the need for only about 135,000 persons, if the productivity of the inmates were the same as that of free labor and if the enterprises of the NKVD were average with reference to output per man. This figure is mentioned here only for the sake of completeness. The output per wage-earner in "unchangeable 1926–27 prices," or, for that matter, in any prices, was much smaller in the industries in which the NKVD mainly participated than the average for all industry, and the figure of 135,000 is evidently too low.

Lumbering was the most important industrial activity of the NKVD. The NKVD share in total procurement of timber including firewood was to be about 12 per cent, according to the 1941 Plan (Table 3). The figure may come to many as a surprise, but it is substantiated by the regional breakdown (1941 Plan, pp. 674–700). The NKVD was not active at all in the lumbering of many oblasts of Central European Russia, as, for example, in Pensa, Ryasan, Tula, etc. In the important Leningrad oblast, the share of the NKVD in lumbering was only 5.0 per cent. In Arkhangelsk oblast, on the other hand, it was 26 per cent, and in Komi Republic, the remotest area of European Russia, more than 50 per cent.

A rough idea of the value and labor input involved in the NKVD activities in lumbering and related industries may be obtained from the data for the Commis-

[9] Michael Romanenko, a Russian technician and a former camp inmate in testimony to the United Nations Economic and Social Council (*Slave Labor in Russia: The Case Presented by the American Federation of Labor to the United Nations* [1949], pp. 58–60), insists that "the average actual productivity of the slave laborer amounts almost to the productivity of the free worker." He supports his statement by the enumeration of means by which this is attained (much longer working days; many more working days in the year; graduated system of feeding; work norms higher than for free labor; punishment by fear). He ends by emphasizing that the NKVD is not only a political but an economic organization, interested in using its slave labor to the extreme. Kravchenko's computations of profits from concentration camps cited below (p. 417) even imply equal productivity of free and slave labor (equal or approximately equal labor productivity may be associated with smaller output per slave laborer, if his equipment is not as good). According to Dallin (*op. cit.*, p. 105), "the average efficiency of a slave laborer has certainly been less than 50 per cent of that of a free Russian laborer." See n. 12 below on the type of evidence used by Dallin.

[10] The "unchangeable 1926–27 prices" are a poor yardstick, because they actually are prices of any year beginning with 1926–27, when the output of each specific good or model started. See chap. iv of *The Soviet Price System* (in press).

sariat for Forestry in Tables 3 and 4. Table 3 reproduces the output of those forestry and related products for which the share of the NKVD is stated in the 1941 Plan. In addition to the items shown, the output of the Commissariat of Forestry also included certain minor items specified in the source and 485.9 million rubles worth of unspecified products; the prices in which this sum was expressed were not indicated, but they were probably in the "unchangeable 1926–27 prices."

TABLE 3

OUTPUT OF SELECTED FORESTRY AND RELATED PRODUCTS AS PLANNED FOR 1941*

Product and Operation	Total	Commissariat for Forestry	NKVD
Felling:			
Timber including firewood (million cubic meters)	290.5	139.6	34.7
Timber proper (million cubic meters)	151.8	82.8	20.6
Transport from the forest:			
Timber including firewood (million cubic meters)	285.1	142.7	37.0
Timber proper (million cubic meters)	151.6	85.0	21.9
Lumber:			
Million cubic meters	28.9	18.8	2.8
Veneer:			
Thousand cubic meters	147.0	145.1	.....
Furniture:			
Total	.....	.....	.....
Million rubles at 1926–27 prices	.....	272.4	.....
Million rubles at wholesale prices	.....	.....	189.3

*1941 Plan, pp. 67–69.

The NKVD output of furniture alone seems large in Table 3. But it was expressed in 1940 wholesale prices, which were high owing to both large turnover taxes and inflation. The bulk of the furniture was actually expected to come from the industrial co-operatives, not shown in Table 3; and their output was to amount to 844 million rubles at 1932 prices (note that the prices are different for each type of producer). The NKVD share in furniture production was only a small percentage of the small total output. Assuming that the "unchangeable 1926–27 prices" of furniture were in 1940 twice the real 1926–27 prices, the 1932 current prices two and a half times this level, and the 1940 wholesale prices eight times as high, the share of the NKVD in total output of furniture was equivalent to only about 6 per cent; it made up about 18 per cent of the output of furniture by the Commissariat of Forestry.[11]

TABLE 4

LABOR FORCE OF THE COMMISSARIAT OF FORESTRY AS PLANNED FOR 1941*
(In Thousands)

Total	1,304.9
Factories	275.5
Felling and forestry work	911.6
Others	117.8

*1941 Plan, p. 521.

The total value of the output of the Commissariat of Forestry was supposed to be 3.54 billion rubles at "unchangeable 1926–27 prices." This implies for

[11] The assumptions as to the various price levels are based on the analysis in the essays mentioned in nn. 7 and 10.

the NKVD lumbering enterprises a value of perhaps 800 million rubles at those prices.

According to the data in Table 4 on the labor force of the Commissariat of Forestry, the establishments of the NKVD would have needed about 260,000 persons, if the productivity per man in its enterprises were the same as in establishments of the Ministry of Forestry, some 240,000 for felling and other forestry work, and some 20,000 for factory labor. This figure alone is much larger than the 135,000 computed above (p. 410) as needed for the total NKVD industrial enterprises on basis of average data for the industry as a whole.

TABLE 5

MEANS OF TRANSPORTATION IN FORESTRY WORK, COMMISSARIAT OF FORESTRY AND NKVD, AS PLANNED FOR 1941*
(In Million Cubic Meters)

	Total	Mechanical
Commissariat of Forestry	142.7	51.6
NKVD	37.0	.....
Office of Forestry Industry of the GULAG	24.7	.....
Office of Forestry Industry	.....	11.6
Not specified	12.3	.....

* 1941 Plan, pp. 68 and 138

Data of the 1941 Plan on mechanical transport of lumber for the principal producers, shown in Table 5, indicate that at least these operations of the Office ("Upravlenie") of Forest Industry of NKVD were not less mechanized than those of the Commissariat of Forestry, although there is some uncertainty because of the manner of designating the agencies involved.

Thus in one case it is the Office of Forestry Industry of the NKVD and in the other case specifically that of the GULAG, but the same agency may have been involved. The data in Table 5 pertain to mechanization of transport, i.e., to replacement of horses, not men, by machines. It is possible that human labor was mechanized much less than horse labor even in the same NKVD enterprises.[12]

The Soviets have not released any data on their gold output for years. The statements of the former inmates of concentration camps imply a very high total, but none of them rests on adequate evidence. *The Statistical Yearbook of the League of Nations, 1941–42* (p. 163) estimated the 1937 gold output of the U.S.S.R. at 136,000–168,000 kilograms, and other estimates for those years are also in the neighborhood of 150,000 kilograms. Some increase in succeeding years is not impossible; but, with war imminent, the Soviets are unlikely to have planned a large increase in gold output in 1941. The amount stated, 150,000 kilograms, is equivalent to 200 million Soviet gold rubles at the 1926–27 parity. This price was probably still used as the price of gold in "unchangeable 1926–27 prices" in the 1941 Plan, although one cannot be sure.

Neither is the gold output scheduled specifically for the NKVD in the 1941 Plan known from official sources. The inmates' estimates and estimates based on inmates' evidence usually run high. Nikolaevsky's estimate of the yearly gold output of the DALSTROI is "about 11 million ounces" in the second half of the

[12] In the book written by Dallin and Nikolaevsky, Dallin emphasizes that "forced labor is . . . being used in those branches of Russian economy where simple manual labor, involving no expenditures for modern machinery, can be profitably employed, for example, in forestry" (*op. cit.*, pp. 89–90). Dallin's evidence that production in NKVD enterprises was performed almost exclusively by hand is limited to a project involving 1931–33 and another one pertaining to 1930.

thirties and 16–18 million ounces in post-war years.[13] Eleven million ounces is equivalent to 315,000 kilograms. Elinor Lipper's estimate for the DALSTROI is 400,000 kilograms, apparently during or after World War II.[14] Vladimer Petrov is an exception when he writes: "DALSTROI had fallen far behind the gold-output plan—it was said that the 1939 plan was for 250 tons [250,000 kilograms] of gold."[15]

Large quantities of gold could hardly be exported without being noticed in the highly centralized world channels for this commodity. No large Soviet exports seem to have been noticed. The idea that year after year the Soviets would have produced huge quantities of gold just for storing seems most dubious. The domestic consumption of gold in the U.S.S.R. appears negligible. I estimate the output of gold with camp labor by the NKVD enterprises around 1940 at 100,000 kilograms, or 133 million rubles per year, for what the estimate may be worth.

Elinor Lipper placed the gold output per person directly engaged in the operation, i.e., without the auxiliary personnel, at 2 kilograms per season of 100 days.[16] This seems to be a generalization from exceptionally favorable conditions. While no other specific evidence is at hand, it is preferable to use the official evidence on the other mining in 1926–27, the year in whose prices the Soviet industrial output was supposed to have been expressed in the 1941 Plan. This evidence suggests an output of only about 900 rubles, or two-thirds of a kilogram of gold per free laborer per year.[17] Actually an even smaller figure seems not impossible on general considerations as to the type of work involved, type of labor normally engaged, etc.

A total output of gold with camp labor by the DALSTROI of 100,000 kilograms per year in conjunction with an output of two-thirds of a kilogram per person per year implies for the gold enterprises of the NKVD the need of 150,000 persons at the labor productivity of free workers. With the substantial adjustment for the lower productivity of camp labor, made below, and with consideration of the auxiliary personnel taken into account here and not considered in the estimate of 200,000 persons made by Elinor Lipper, my estimate of camp inmates engaged in gold enterprises of the DALSTROI substantially agrees with hers. My estimate also agrees with the estimate of 400,000 total population in DALSTROI's camps in those years made by W. Andrews.[18]

The other NKVD industrial activities mentioned in the 1941 Plan were as follows: (1) 5.3 million tons of coal out of a total of 191 million tons (p. 13); (2) 250,000 tons of oil out of a total of 35 million tons (p. 15); (3) 100,000 tons of cement out of a total of 7.8 million tons (p. 61); (4) 82 million bricks[19] (p. 727); (5) 150,000 tons of chrome ore out of a total of 370,000 tons (p. 17); and (6)

[13] *Op. cit.*, pp. 145–46.

[14] *Eleven Years in Soviet Prison Camps* (Chicago, 1951), pp. 103–4.

[15] *Soviet Gold* (New York, 1949), p. 369.

[16] *Op. cit.*, pp. 103–4.

[17] In 1926–27 the output per worker and year in other mining operations was as follows (in rubles):

Coal	783
Iron ore	1,044
Copper ore	887
Silver-lead	757

(see Central Statistical Board, *Statistical Handbook U.S.S.R., 1928*, pp. 316–17 and 544).

[18] *An Army in Exile* (London, 1949), p. 75. This estimate is 400,000 in camps, including 357,000 able-bodied persons, and 50,000 free population.

[19] GULAG in Primorskii krai and Khabarovskii krai only. The share of the NKVD in the brick output of the other areas was not stated.

59,400 tons of fish caught out of a total of 1,661,400 tons (p. 73).

All listed activities are, of course, small. At the average "unchangeable 1926–27 prices" and the average labor productivity for the whole respective output, the values and labor inputs which would have been involved in the more important among enumerated secondary NKVD activities are shown in Table 6. One hundred thousand tons of cement

TABLE 6

Item	Values in Million Rubles at "Unchangeable 1926–27 Prices"	Yearly Workers (Thousands)
Coal	70	20
Oil	25	2
Fish	36	3
Chrome ore	1	1

may have had a value of 7 million rubles, and 82 million bricks of 5 million rubles (real 1926–27 prices).

If the findings on the industrial activities of the NKVD, thus far made, are totaled, they yield a value of about 1.2 billion rubles at "unchangeable 1926–27 prices" and about 440,000 persons at the productivity of free labor. It was shown, however, that the total industrial output scheduled for the NKVD was to amount to almost 2 billion rubles at "unchangeable 1926–27 prices." Hence some 800 million rubles' worth of industrial output remain still unaccounted for. The unaccounted-for industrial activities of the NKVD included its shares in nonferrous metals other than chrome ore as well as other commodities which were not listed in the plans, and its shares in outputs listed in the 1941 Plan but with the share of the NKVD not specified. It seems doubtful that all these activities amounted to as much as 40 per cent of the total industrial output of the NKVD, unless the balance consisted largely of goods, greatly overvalued in terms of "unchangeable 1926–27 prices." Considerable overvaluation in terms of "unchangeable 1926–27 prices" would naturally imply a correspondingly large output per worker in terms of those prices. Moreover, the output per worker in the unaccounted-for industrial activities of the NKVD is in any case likely to be considerably higher than in the listed activities because the latter, namely, forestry work and mining, are among those with the lowest values of output per worker. It will be assumed that 550,000–600,000 wage-earners would have been needed for the industrial output of the NKVD, had it been produced with free labor. It may have required 1,000,000 concentration-camp inmates.

Other and total economic activities.—No activities of the NKVD except in construction and industry are shown in the 1941 Plan, although one would expect at least some agricultural activities, such as growing potatoes and vegetables for the inmates (such things as milk or eggs are apparently no part of their diet). Margaret Buber (Neumann) worked on a large farm attached to a camp in Karaganda (Kazakhstan).[20] Elinor Lipper enumerates five farms in the Kolyma area engaged mainly in cabbage growing.[21]

The NKVD was also to some extent engaged in shipping, at least in the Far East. This is obvious from the new ships provided in the 1941 Plan for the

[20] *Under Two Dictators* (London, 1949). They even had a grain combine-harvester (see, e.g., p. 106) but did not produce enough to feed the camp inmates.

[21] *Op. cit.*, p. 100.

NKVD (pp. 88–93). They are as shown in Table 7.

The number of concentration-camp inmates employed in economic enterprises besides construction and industry will be arbitrarily estimated at 150,000.

The number of concentration-camp inmates to be employed in all economic activities of the NKVD according to the 1941 Plan, as estimated above, is summed up in Table 8.

TABLE 7

Craft	Number
Screw tow boats:	
800 hp.	1
Up to 400 hp.	2
Metal sea launches:	
65 hp.	5
60 hp.	12
50 hp.	6
Metal barges: 150 tons capacity	3
Metal coal barges: 500 tons capacity	5
Metal oil barges: 400 tons capacity	1
Wooden sea barges (own output):	
150 tons capacity	15

TABLE 8

ESTIMATE OF INMATES TO BE EMPLOYED IN NKVD ENTERPRISES IN 1941

(In Thousands)

Construction	1,172
Industry	1,000
Others	150
Total	2,322

TOTAL NUMBER OF CONCENTRATION-CAMP INMATES

As stated, the 1941 Plan does not contain any evidence on the number of inmates hired out for work in enterprises other than those of the NKVD. A trace of this labor may possibly be found in the fact that the average yearly wage of workers proper (i.e., not including employees) in felling and forestry work of the Commissariat for Forestry was expected to be only 2,240 rubles, i.e., only a little more than half of the average wage of a worker. Workers employed in lumbering were always very cheap in Russia and the U.S.S.R. because most of them were peasants, all too glad to find outside work in the winter. There was also semivoluntary or fully compulsory recruitment of kolkhoz peasants for forestry and other work. Still, the average wage of the workers proper of the Commissariat of Forestry employed in felling and forestry work might possibly not have been as low as 2,240 rubles, if part of the workers had not been concentration-camp inmates.

The available evidence indicates that using the concentration-camp inmates in the NKVD's own enterprises is the rule, and hiring them out to other enterprises is the exception. This is at least the opinion of George Denike, who carefully studied the available literature, and of others consulted. Assuming that 20 per cent of the employed concentration-camp inmates worked in enterprises other than those of the NKVD, the figure of 2,322,000 computed above for the inmates employed in the enterprises of the NKVD rises, after rounding, to 2,900,000.[22]

All construction of camps and in camps is listed under capital investment in Soviet statistics. From there they enter the so-called basic funds—this measure of riches of the "socialist state." The NKVD labor employed in this con-

[22] F. Beck and W. Godin, *Russian Purges and the Extraction of Confessions* (New York, 1951), p. 78, write: "The GULAG hired out the labor force at its disposal to Soviet undertakings. . . . Nearly every big factory and enterprise in the country had detachments of prisoners working for it. Many industrial enterprises, particularly in Siberia and other distant areas, could not have kept going without them." The authors were fair to mention that theirs was not firsthand information, because "both authors spent their entire time in a remand prison" (*ibid.*, p. 77).

struction was already included above under construction. Labor used in industry, agriculture, shipping, and other gainful enterprises was also considered. There still remain, however, considerable numbers of concentration-camp inmates used for servicing those who are employed in gainful occupations, such as office clerks, cleaners, cooks, medical personnel, etc., and even internal police. With those persons considered, the total number of concentration-camp victims used for work may have been as high as three and a half million.[23] This figure does not include children, full invalids, people too old to work, etc. I have no evidence by which to estimate them.[24]

CHECKS

The figure for concentration-camp inmates arrived at here may seem small as compared with the usually cited exorbitant figures. I did not accept the latter even before I had access to the 1941 Plan. Figures of ten to fifteen million in concentration camps just do not make sense. They do not fit into the whole picture. The actual figure may have been even smaller than my estimate.

Available evidence leaves no doubt that most inmates are adult males. There were only about forty-five million men twenty to fifty years of age in the U.S.S.R in 1940, and such a figure as fifteen million for all concentration-camp inmates would imply more than 25 per cent of all men of this age in those camps. The proportion of men twenty to forty years of age would be even larger.

Another check is this. Practically all camp labor was used on construction and industry. The role of the NKVD was moderate in *total* construction and almost negligible in total industry. According to the 1941 Plan, the total number of persons engaged in those activities, excluding those in the NKVD projects and enterprises, was expected to be 14.1 million persons. The number of concentration-camp inmates employed in these economic pursuits as computed above appears very large in relation to this figure. It is justified only by the (here accepted) low labor productivity of the inmates.

The persistent failure of the Soviet goals for the output of timber and lumber supports my computations. The goal of the fourth Five-Year Plan for the output of industrial timber was missed by as much as 27 per cent, and the situation in the last years before World War II was similar. This would not likely have been the case if the number of concentration-camp inmates had been greatly in excess of my estimates.[25]

[23] Some of the activities for which provision is being made here are certainly included under economic activities in Soviet statistics. The NKVD canteens, for example, mentioned by many former inmates, are likely to be part of "socialist public feeding" and hence are included in statistics under trade. This does not make any difference for the purposes of the present analysis, because no provision for trade was made in estimating the labor force in the NKVD economic enterprises. But, for all I know, Soviet statistics may have included every NKVD tailor and shoe-repair shop under industry. Since the labor requirements of all NKVD industrial enterprises were already included above, the possibility of a certain amount of double counting is present.

[24] David Dallin, who read an earlier draft of this paper, believes that the number of inmates turned over by the NKVD to other agencies may have been greater than the estimates made here. He mentions furthermore that, according to a person who was chief of police of a large camp, only 50–60 per cent of all inmates are engaged in "productive" work. The rest are in hospitals; are invalids; are used in internal police, bakery shops, bathhouses, etc. I can only repeat the reservation made introductorily: All computations made here are subject to a wide margin of error. This pertains especially to the estimates of those not engaged in the economic enterprises of the NKVD.

[25] By a procedure for which I can develop no enthusiasm, N. S. Timasheff estimated the number of persons in prison camps on January 1, 1938, at two million (see his "The Postwar Population of the

CONCENTRATION CAMPS A LIABILITY

Statements are made, time and again, that the Soviet government derives big profits from exploiting the labor of concentration-camp inmates. By a procedure which reminds one of those common in the country of the "beloved leader," Victor Kravchenko arrived at the conclusion that during 1938–40 the NKVD got as profits from the camps a minimum of 27 billion rubles per year. Together with profits from those penalized to work at their jobs at reduced wages, the profits from concentration camps in those years were roughly equivalent to 85 per cent of the total investments, according to Kravchenko. In 1941–43 they amounted to 114 per cent of the latter. Kravchenko counted "at least fifteen millions of persons in labor camps" and a daily profit of six rubles per man, of which he said "we probably are assuming a figure far below the actual."[26]

A profit of 27 billion rubles is supposed to have been obtained from construction worth some 6.5 billion rubles gross—i.e., including raw materials, etc.;[27] industrial output worth about 2.0 billion rubles gross at "unchangeable 1926–27 prices"; and what the NKVD could net from the pay of other organizations for their use of concentration-camp labor—an elephantine exaggeration. All such statements are possible only because no attempt is made to fit the camps into the whole Soviet picture.

I firmly believe that concentration camps are a liability rather than a source of profits. Those who insist on their great profitableness should stop and think of the reasons for the very low consumption levels of the "free" population, of the miserable real wages of workers and even poorer incomes of peasants. If the concentration camps had covered all or most of the investment, the "free" population would have a better lot. With the huge capital investments (and "defense" expenditures) falling on their shoulders, the "free" laborers have to be starved. The concentration camps, indeed, actually diminish the number of those who shoulder the proportionate burden of those huge expenses.

The Soviet Union is badly in need of all kinds of skilled labor. It spends heavily on gratuitous education. If even part of the skilled laborers taken into the camps are then turned to unskilled work, this involves a considerable loss. Beyond any doubt mortality is far greater inside the camps than outside. This is a great additional loss to the state, which is almost the only entrepreneur in the U.S.S.R.

It is safe to assume that not all projects performed with forced labor would be undertaken, if full wages had to be paid. There must also be considerable waste of time involved in moving inmates from camp to camp, in not having the work ready to start when the inmates are on the spot, etc.

Those who assume big profits from concentration camps do not take all the adverse factors into consideration. Whether they think of them or not, their starting point seems to be: "There is an unlimited number of persons who can be put in the camps; it costs nothing to obtain them." There was a period in Roman history when wars were so frequent, the number of prisoners obtained was so large, and their price so low that it was not profitable to raise slaves. It was bet-

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Soviet Union," *American Journal of Sociology*, LIV [September, 1940], 150), but in spite of the procedure, he came to a much more realistic appraisal than did many others.

[26] *I Chose Freedom* (New York, 1950), pp. 335–36.

[27] The estimate of 6 billion rubles made above (p. 409) has been raised to 6.5 billion to allow for construction work connected with repairs.

ter to buy them in the market for a few dollars each,[28] draw out of them all their strength, let them die, and go again into the market for others. This reasoning may be valid for the Soviet concentration camps so far as they are filled with foreigners (their number is also limited). But the adults of the U.S.S.R., especially skilled ones, represent an investment which is better exploited outside than inside the regular camps.[29]

In the foregoing computation it was assumed that, including the auxiliary staff, somewhat more than two concentration-camp inmates were needed to do the work of one free laborer. If the concentration labor was more productive than this, the total number of concentration-camp inmates was correspondingly less, as was the number of "profit" yielders. With the ratio of more than two to one,[30] and the cost per inmate assumed by Kravchenko, the profit per man, as he computed it, disappears. Indirect losses involved in removing the people from their former occupations, which were really profitable for the state, increased mortality, etc., turn the alleged profits into losses.

The only economic advantage from concentration-camp labor that I am able to see is the opportunity to move this labor to places and to use it for work where and for which free labor would be difficult or impossible to obtain. The government is, for example, compelled to pay free labor in Pechora coal mines 80 per cent more than in the Donbass coal mines. But the whole Pechora coal enterprise is a very dubious economic enterprise. The same is true of most of the Soviet gold enterprises and of many others based on concentration-camp labor.

CONCLUSIONS

The status at the end of 1940 is unfortunately the only one for which even a crude estimate of the number of inmates in the camps, an estimate of the type here made, can be undertaken. The 1940 contingent of inmates was probably swelled by newcomers from the territories "liberated" by the Soviets after the start of the war outside of the U.S.S.R. in mid-1939. The war brought an inflow of prisoners of war which greatly overcompensated for the Poles who, now as allies, had to be set free, so far as they were still alive. The end of the war brought a wave of Soviet soldiers who had been abroad and who had to be purified from the poisonous atmosphere of capitalism before being admitted into the socialist Eden. There were also quite a few peasants, who took for real money the promises of relaxation on the kolkhoz front given during the war, and needed a thorough cooling off. After 1947 a fresh wave came into the camps from the newly acquired territories. The peasants of these areas saw the light. Almost overnight they realized the great advantages of the large-scale agricultural production —on their land and with their livestock. In a few months most of them were collectivized. Those failing in enthusiasm

[28] Tenney Frank gives the normal price of a work-slave in the last two centuries of the Republic and the first two centuries of the Empire as 500-denarii, equivalent to about $13 (see his *An Economic Survey of Ancient Rome* [Baltimore, 1940], V, 235). Slaves were much cheaper during short periods.

[29] Dallin was undoubtedly inconsistent when, on the one hand, he said: "The average efficiency of a slave laborer has certainly been less than 50 per cent of that of a free Russian worker" (*op. cit.*, p. 105), and recognized "the tremendous waste of human material in the camps" (pp. 113–15) and, on the other hand, assumed that the NKVD realizes "huge profits" in certain of its enterprises and that those profits are "an important element of the government industrialization fund" (*op. cit.*, p. 88).

[30] Including the personnel servicing those engaged in gainful operations.

had to be indoctrinated in the camps. At the end of 1949 the big Khrushchov campaign for great enlargement of the kolkhosy, creation of agro-cities, and the reduction of the kolkhozniki's private gardens started all over the U.S.S.R. It certainly was accompanied by an accelerated flow of peasants from both old and new territories into the concentration camps.

In short, the stream of those in need of "correction" does not dry up. The flow is kept alive by new measures intended to guide the blessed on the path from socialism, already reached, to communism, which is just around the corner. The "corner" is the per capita labor productivity of the United States—a formidable obstacle in view of the fact that at the moment it is a few times that of the U.S.S.R. No attempt will be made to estimate the precise number of the citizens of the only "true democracy" who are in the camps at the present time.

A LOAN RATIO FOR INFLATION CONTROL

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I

THE public has persuaded itself (or has been persuaded) that the inflation under way is greater than can be accounted for on monetary grounds. Profiteering, the wage-price spiral, the hoarding of inventories, and the federal deficit are four factors, allegedly nonmonetary, invoked to explain the course of events. Specific controls over prices, wages, and allocations are believed necessary to control inflation. Logically so, if in fact the inflation is greater than monetary expansion can explain.

But if the figures point the other way, if they indicate that inflation is *not* proceeding more rapidly than monetary expansion, if profiteering and hoarding and spiraling have affected prices only through forcing more money into the economy and making it circulate faster, perhaps the opposite implication holds. Rather than a network of specific controls and ceilings, perhaps a single ceiling on monetary expansion is required for a mobilization economy.

Such monetary control, if feasible at all, is eminently desirable. It would require only a small staff for enforcement and administration. Operating on an over-all level, it would involve only an infinitesimal threat to personal and economic liberties, even should it become permanent. Who can say as much for direct controls?

This essay has three purposes. (1) It is designed to show that post-Korea inflation is explainable on orthodox, old-fashioned monetary grounds. (2) It is proposed to define an index or ratio which can be used as a major weapon of monetary policy and which may permit the Federal Reserve System to halt the inflation in its tracks. (Given intestinal fortitude to overcome interests which have become vested in present and higher price levels, we may be able to use the ratio further to roll the price level back. This without sacrificing government credit, without extortionate interest on the public debt, and with only temporary setbacks to production or employment.) The ratio in question I am calling the "loan ratio."[1] (3) The paper is designed, finally, to consider certain objections to the "loan ratio" as a monetary policy instrument and to answer them as far as possible. Comparisons with alternative plans will be made only incidentally.

II

Let us take as our statistical base the month of May, 1950, the month before the outbreak of the Korean war. A few wavelets of renewed inflation have put in their appearance,[2] but the price level is

[1] This name was suggested by my colleague, Professor Walter A. Morton. For further suggestions I am indebted to a number of other colleagues, particularly James S. Earley, John Gronouski, and Theodore Morgan. None of these colleagues, however, should be held responsible for any policy suggestions which I shall make regarding this ratio.

[2] Some such inflation was nearly inevitable, by reason of the government forestalling of any threatening recession in 1949 by resuming deficit financing and practically guaranteeing "full employment at whatever cost" (the phrase is Professor Jacob Viner's, *Quarterly Journal of Economics*, LXIV [August, 1950], 385).

reasonably stable and well below its 1948 peak. Using May, 1950, figures as a base, certain selected economic indicators have moved as shown in Table 1 by January,

TABLE 1

Series	January, 1951, Value (May, 1950 = 100)
Commercial bank loans	120.0
Wholesale prices	115.5
Turnover of demand deposits (outside New York City)	114.6
Consumers' prices	107.7
Money supply	106.4

1951, when over-all controls are first imposed.[3]

Two important points should be noted at the outset. Commercial bank lending, the volatile element in the money supply, has shot up more rapidly than any of the other series. Its expansion has been well ahead of wholesale prices, let alone the lagging cost of living. In addition, the rapidity with which money was being used seems almost to have kept pace with wholesale prices without requiring any monetary expansion whatever. The conclusion seems clear: but for technological improvements, and some increase in the labor force, the rise of bank lending and the accelerated turnover of money would have supported a greater price rise than actually occurred.

At the same time, Treasury operations, which have been an easy target in assigning blame for the inflation, have been positively deflationary in their immediate impact. Inflationary commitments for the future have been made, and Congress has been slow to cover them with appropriate tax increases, but the cumulative cash position of the Treasury exercised a contractive influence during this period. Over nine months beginning May 1, 1950, the Treasury has received from the public $1,927 million more than it has paid out. If a scapegoat for the inflation must be found, our conservative bankers fit the role much better than our welfare-state government in Washington.

Furthermore, this was not the first recent American inflation fed primarily by bank lending in the face of government contraction. In the three fiscal years 1947–49 inclusive, a Treasury cash surplus of $16,613 million was offset almost precisely by an expansion of $16,405 million in commercial bank loans,[4] while wholesale prices rose from 105.0 to 155.8 on a 1926 base.

The Korean war inflation, moreover, was accompanied by a rise in production, not a fall. The index of industrial production, in particular, rose 24 points (12.4 per cent) between May, 1950, and January, 1951. This is encouraging in itself but runs counter to the widespread superstition that higher production will solve the inflation problem unassisted. Indeed it may, if it can come about without generating equivalent increases in consumers' incomes. In practice, it al-

[3] The statistics involved in these calculations are available in the monthly *Federal Reserve Bulletin*. I shall make no specific page references, but certain comments may be in order:

a) Seasonal adjustments have not been made. They seem unnecessary in view of the smoothness of most of the upward movements.

b) Statistics referring to bank loans are sample estimates except at semiannual call dates. They include all commercial banks in the United States, including state banks not members of the Federal Reserve System. They do not cover mutual savings banks and similar institutions.

c) Interbank and government deposits are not considered in computing turnover of demand deposits.

d) "Money supply" includes both outside currency and demand deposits. It excludes time deposits, interbank deposits, foreign bank deposits, and Treasury balances. It is somewhat more sensitive than the usual total, which includes time deposits.

[4] The cash surplus covers the period April, 1946—March, 1949; the bank loan expansion the period January, 1946—December, 1948.

ways does generate such increases, and the increases are nearly equivalent, apart from the slow progress of technology and a few secondary items like additions to business cash holdings. As present experience shows, we can have rampant inflation with full production, just as we suffered rapid deflation with idle industrial capacity in the early 1930's.

III

It is not ridiculous, then, to look on the present inflation as primarily a monetary phenomenon and to consider controls primarily from the monetary side. Such controls might be designed, for example, to stabilize a price level by adjusting the quantity of money so as to offset changes in the real volume of goods produced and in the velocity with which money is being spent.

More volatile than any of the economic series listed in the last section, and showing more clearly the extent of credit inflation, is the ratio:

$$\frac{\text{Commercial Bank Loans}}{\text{"Original" Demand Deposits (Not Resulting from Commercial Bank Loans)}} \qquad (1)$$

This is at once more sensitive and more meaningful than the ordinary ratio of loans to deposits. It excludes from its denominator the "derivative deposits" which arise on the books of commercial banks in consequence of the loans they make. The ratio of loans to derivative deposits is always close to unity for the commercial banking system as a whole, although it is much less for any individual bank. The exclusion of derivative deposits brings out in sharper relief the expansive or contractive effect of bank loan policy. The "loan ratios" on which I shall propose ceilings are based upon expression (1).

Unfortunately, it is impossible to distinguish "original" from "derivative" deposits in normal banking practice. Deposits carry no tags around their necks; they are not dogs, and neither are they infantrymen. Suppose that A, who has borrowed from the Impregnable National Bank, uses the proceeds to buy raw materials from B. B credits the receipts to his account, which happens to be at the Imperturbable Bank of Commerce. The resulting deposit is to the Imperturbable as "original" a deposit as one could wish. As a rough estimate of "original" deposits, however, we can take the difference between total demand deposits (excluding interbank) and total loans.[5] This can apply equally well for a single bank and for the entire commercial banking system.

In concrete form, then, the suggested "loan ratio" may be written:

$$\frac{\text{Commercial Bank Loans}}{\text{Commercial Bank Deposits (Other than Interbank) } \textit{Minus} \text{ Commercial Bank Loans}} \qquad (2)$$

Observation of the behavior of the ratio (2) for the entire commercial banking system will indicate the extent of bank loan expansion which has taken place. It has taken place despite warnings, preachments, and exhortations from the Federal Reserve System, the American Bankers Association, and other men of good will opposed to inflation. We can hardly expect programs of "voluntary restraint" to alter this trend for so long a period as the duration of the mobilization crisis (Table 2).

There is something ominous about the advance of this ratio. It has moved with the rhythm of an economic dirge. It is a

[5] Strictly speaking, "original deposits" should also exclude those deposits which arise from the purchase of government or private securities by commercial banks. The necessary adjustments are not being proposed here, since we desire to make a bank's loan ratio as exclusively dependent as possible upon its lending policy.

measure of inflation in being. It is at the same time a forerunner of inflation still to come, since the velocity of circulation is creeping upward at the same time. A reversal of this advance, I submit, and a sufficient decline, will suffice to check and reverse the trend of the inflation. It can put a ceiling on the price level—either at its present height or at some lower point. It can render superfluous much of the elaborate mechanism of direct price, wage, and allocation controls currently in process of erection.[6] In short, ceiling control of banks' loan ratios would make most other ceilings unnecessary and should become one of the foundations of monetary as distinguished from fiscal policy.

TABLE 2

(Dollar Figures in Millions)

Month 1950–51	Commercial Bank Loans	Demand Deposits	Denominator of (2)	Loan Ratio	
				Value	Index
May	$44,080	$ 94,150	$50,070	0.880	100.0
June	44,796	95,485	50,689	0.884	100.5
July	45,980	96,040	50,060	0.918	104.3
Aug.	47,270	97,180	49,910	0.965	109.7
Sept.	49,030	98,040	49,010	1.000	113.6
Oct.	49,890	98,790	48,900	1.020	115.9
Nov.	51,650	100,130	48,480	1.065	121.0
Dec.	52,830	104,090	51,260	1.029	116.9
Jan.	52,890	101,310	48,420	1.092	124.1

Loan-ratio control is, it must be admitted, a one-way street. When the economic problem is one of falling prices, depression, and underemployment, raising loan-ratio ceilings or removing them completely (as I advocate) will do no good. Among the lessons of the 1930's is the inability of legal incantation to increase the volume of credit when neither borrowers nor lenders are sufficiently sanguine to expand on their own volition. And on the inflationary side as well, which is the immediate issue, deficit financing can undo loan-ratio controls faster than loan-ratio ceilings can be lowered. During World War II in the United States, for example, no control was required to reduce the loan ratio drastically, but at the same time the groundwork for postwar inflation was being laid.[7] This was one result of financing the war effort so largely by the sale of bonds to the banking system. The commercial banks paid for the bonds by creating new deposits for the Treasury. These new deposits appear as original deposits in the denominator of our ratio and thereby lower automatically the value of the ratio itself.

There is, moreover, no precise or unique relationship between the loan ratio and any price index number. This it is not necessary to maintain, and I do not maintain it. The proposal here is only

[6] Some rationing controls, particularly at the consumer level, will probably remain desirable from a *morale* or *distributive* point of view. Assent of the debtor masses to any sound anti-inflationary program is difficult to secure. It will probably require assurance that cuts in consumption of scarce goods be shared more equally than they would be shared under an exclusively monetary and fiscal program. If the rich do not require consumer credit, and can pay their taxes out of savings or capital, they will not have to reduce consumption when credit is restricted and taxes raised. If the poor finance large purchases on credit and pay their taxes out of consumption, it is they who will cut their living standards immediately under a monetary-fiscal inflation-control program. The implications of such a program are obvious. The poor will cut consumption, and the rich will consume the scarce items. Rationing has a place in remedying the resulting inequities.

[7] Professor Earley has computed year-end values (dollar figures in billions) for the loan ratio during the years 1940–45 inclusive, which bring out this point very clearly:

Year	Loans	Demand Deposits	Denominator	Loan Ratio
1940	$18.8	$ 38.6	$19.8	0.95
1941	21.7	44.3	22.6	.96
1942	19.2	61.4	42.2	.45
1943	19.1	75.6	56.5	.34
1944	21.6	91.7	70.1	.31
1945	26.1	106.0	79.9	0.33

that the American commercial banking system be required, within a sixty- or ninety-day period, to reduce its loan ratio to an over-all average of, say, 1.05. (The grace period is necessary to avoid violation of short-term loan contracts already in existence.) Following this initial reduction, the ceiling ratio should be reduced by, say, 2.5 points each month thereafter for so long as the wholesale price level is above a certain figure which should be announced in advance. (I suggest 155 or 160 on a 1926 base; the May, 1950, quotation was 155.9.) This could be done within a single year, and the economy returned to a stable and healthy condition.

A return to a loan ratio of 0.85 or 0.90 would probably not mean a return to the precise pre-Korea price level. The increased output and employment might result in a lower level; more likely, it would have been overbalanced by habits of more rapid monetary turnover. The lower price level rather than the lower loan ratio, in my opinion, should be the criterion of policy. Yet in any case, return to a lower loan ratio would mean a whipped inflation, unless deficit financing took over. It would mean an end to the speculative hoarding of unsold goods on borrowed money. It would mean a stabler and healthier economy. The process would not be painless by any means. There would be stringencies and temporary checks to both production and employment. The process of adjustment would be strong medicine but worth its cost in a fairly short run. Of that, however, more below, in Sections VI–VII of the present paper.

There are unfortunately wide divergences concealed in the national averages among the loan ratios for individual banks and classes of banks. As of November 29, 1950, the loan ratio (2) was computed for several classes of Federal Reserve member banks, with a spread as shown in Table 3.

The loan ratios vary from over 5.0 in the St. Louis district to below 0.8 in the large Chicago banks. They seem higher in small cities than in either metropolitan centers or rural areas. Lending policy also appears somewhat more liberal in the South and West than in the East and Midwest. In view of the disparities shown by the table, it is clearly impractical to force individual banks into any uniform mold or pattern based on a national average.

TABLE 3

New York City (Central Reserve city banks)	1.097
Chicago (Central Reserve city banks)	0.760
Reserve city member banks	1.251
Country member banks	0.899
Weekly reporting member banks, by Federal Reserve districts:	
Boston	1.138
New York	1.502
Philadelphia	1.059
Cleveland	1.018
Richmond	1.072
Atlanta	1.506
Chicago	0.874
St. Louis	5.139
Minneapolis	1.841
Kansas City	1.474
Dallas	2.048
San Francisco	4.242

Each commercial bank, however, should be forbidden to increase its loan ratio beyond its value as of some specified date in the past (December, 1950, or January, 1951). If a ceiling ratio is set as of this date, it may be rolled back later at monthly intervals. A roll-back should be by a specified number of index points or a percentage of the previous ceiling ratio, whichever yields the larger dollar reduction. Past seasonal patterns in loan ratios could also be taken into consideration at

the option of any bank which so desires, with the ceiling applied to the loan ratio as adjusted statistically for seasonal variation. Special controls, such as a high reserve ratio specifically against post-Korea loan increases,[8] may be desirable in addition, as penalties against banks which have expanded loans most sharply in the first months of the defense emergency. (See, however, the discussion in Section IV below.)

Parallel controls might be extended to savings banks as well, if loan expansion by these institutions became a serious factor in the inflation picture. The ceiling ratio applicable to savings banks would necessarily be different from that for commercial banks, since savings institutions operate characteristically on small cash reserves and hold time rather than demand deposits. Control over bank investments (other than government securities) might also be required, if it became a popular mode of evasion to disguise loans as investments.

IV

There appears to exist authoritative opinion that the loan ratio can be made a subject of Federal Reserve regulation without congressional legislation. It has been suggested, notably by President Truman at a White House conference of February 26, 1951, that the provisions of the Emergency Banking Act of 1933 remain in force. Under this legislation the President, through the Secretary of the Treasury, has blanket power over the conditions under which Federal Reserve member banks may do business. These powers may, it is said, be delegated to the board of governors of the Federal Reserve System. At the same time they may be extended to nonmember banks under authority of the Trading with the Enemy Act of World War I.[9]

Control over the loan ratio is not inconsistent with regulation of bank credit by other devices. It can attack the problem of loan expansion more directly, and be evaded somewhat less easily, than can the other over-all controls with which it might be combined. Four such control devices, some already invoked and others under discussion, are listed below. Advantages of loan-ratio regulation over certain of these plans are also indicated.

1. The ordinary reserve requirements against bank deposits may be increased, requiring the commercial banks to keep increased deposits at the Federal Reserve banks. These increased deposits can to some extent at least be secured simply by selling marketable government securities to the Federal Reserve System at the "pegged" price for these securities. Since the "peg" has been permitted to slip below par, this process involves a capital loss and is not the costless affair it was prior to 1951.

A variant of this plan involves a differential reserve of 90 or 100 per cent on all increases in deposits since some base date, such as the outbreak of the Korean war. This could again be met by sales of

[8] A proposal that reserve ratios be computed against earning assets rather than against deposits, with different ratios against different classes of earning assets (the highest presumably against loans), was made by Professor Viner at the White Sulphur Springs Conference in April, 1951, and amplified in a letter to the *New York Times* dated April 16. As a general regulatory plan, it does not appear to allow adequately for differences between banks. Either the differentials between asset reserve ratios would be too small for effectiveness, or else there would be risk of serious embarrassment to banks which had customarily concentrated on lending rather than investing. As a penalty provision against loan *increases*, however, the plan seems meritorious.

[9] *New York Times*, February 27, 1951. The White House conference in question was attended by representatives of the Council of Economic Advisers, Office of Defense Mobilization, and Securities and Exchange Commission, in addition to the Treasury Department and the Board of Governors.

marketable government securities to the Federal Reserve System, at the cost of appreciable capital loss. A differential reserve of this type would lower the *net* rate of return on *increased* commercial bank lending by somewhat more than would an across-the-board increase in reserve requirements. It would also discriminate against new banks, banks located in growing communities, and banks which are most successful in attracting depositors and other customers from competitors.

This discriminatory feature also militates against any flat ceiling on either the loans or the deposits of any individual bank,[10] or against applying a loan ratio only to post-Korea increases in loan volume. Even when certain banks have expanded loans in defiance of pressure for voluntary restraint, it is difficult if not impossible to penalize them without simultaneous discrimination against new and growing institutions.

The Viner proposal for reserve requirements to be imposed against classes of assets rather than against deposit liabilities, discussed above in note 8, also belongs in this category.

2. Selective or qualitative credit controls may be imposed, particularly over consumer, real estate, and housing credit, and over margin requirements on security loans. The volume of credit affected, however, is relatively slight. Outright evasion is by no means impossible.[11] Potential borrowers can turn to other sources. Thus if both banks and insurance companies make both real estate and commercial loans, and bank real estate lending is restricted, what will be the result? Real estate loans will shift to some extent from banks to insurance companies, and ordinary commercial loans and investments from insurance companies to banks. The total volume of loan credit will remain approximately unchanged.

3. Federal Reserve support for the government securities market may be modified by substantial lowering of the support price, by "pulling the plug" and abandoning support altogether, or by restricting the marketability of future issues of government paper. Expert opinion, particularly in Federal Reserve circles, maintains the existence of some golden mean or happy medium in bond prices and interest rates. This "golden mean" rate and price will dissuade lending agencies from liquidating their existing holdings of governments and yet require from the Treasury no serious burden of interest rate increases when new securities are issued or old ones refunded. As a plain man and in a single station, I doubt the existence of any such rate. The gold in the golden mean rate must come, it would seem, from the mythical Seven Cities of Cibola, in a period when expectations about future interest rates are, in jargon, "elastic upward." That is to say, any Treasury concessions on interest rates in the present are apt to be taken, when expectations are elastic upward, as promises of bigger and better increases to come, at least until the rate has risen sufficiently to add several billions a year to the cost of federal debt service.[12]

[10] A plan of this type was outlined in the *Wall Street Journal*, March 17, 1951, authorship being ascribed to Dr. John D. Clark of the Council of Economic Advisers.

[11] Evasion occurs, for example, when a loan made for an uncontrolled purpose frees the borrower's own capital for use in consumer or real estate credit, or for the purchase of securities. Banks usually attempt to check such evasive activities but can never be completely successful.

[12] Perhaps the best treatment of the interest rate problem as regards American government securities remains a relatively neglected article by Lawrence

4. Commercial banks may be required to hold a certain percentage of their deposits in government securities. There would then be a "security" or "secondary" reserve in addition to the "cash" or "primary" reserve required under existing law. The secondary reserve could be held in existing issues, or it might be limited to a special issue bearing a lower interest charge.

This proposal was developed by Professor Seltzer to avoid the inflation he foresaw as a result of the coincidence of substantial excess bank reserves and the heavy defense spending which preceded America's entrance into the second World War.[13] It has since been recommended intermittently in the Federal Reserve System, being associated with the name of former Chairman Marriner S. Eccles of the Board of Governors and former Chairman Simeon E. Leland of the Federal Reserve Bank of Chicago.[14] It has been opposed by commercial bankers fearful of its effect on bank earnings and has never aroused much congressional support.

An advantage of the loan-ratio ceiling over the secondary-reserve proposal is that it affects loans directly. Under the secondary reserve, a bank wishing to expand its loans would be less able to do so by selling its government securities but could instead dispose of other investments, perhaps at somewhat greater risk of capital loss. Under the suggestion proposed here, control over loans, which we suppose less inflationary than investments, would be significantly tighter.

V

Objections to a loan-ratio ceiling can be expected from three sources. They will come almost inevitably from bankers. No one likes to see his business subjected to increased outside control, and bankers

H. Seltzer, "Is a Rise in Interest Rates Desirable or Inevitable?" *American Economic Review*, XXXV (December, 1945), 831–50.

In March, 1951, the Treasury offered to exchange a limited amount of existing 2½ per cent marketable long-term securities for a new 2¾ per cent issue, marketable only indirectly and at substantial loss. The exchange is at the option of the holder, not the Treasury. It compensates for decreased marketability by a rate increase of only ¼ per cent. At the same time, the price of the existing marketable 2½'s has been permitted to sag slightly below parity, so that their effective rate of return is also approximately 2¾ per cent.

There are three alternative reactions to this offer, if it is enlarged and generalized. (1) Some holders will accept the exchange. (2) Others will gamble on the 2½'s being pegged sufficiently close to parity to make the added ¼ per cent of return insufficient compensation for the loss in marketability. These will continue to hold the 2½'s. (3) Still other holders will anticipate further decline of the 2½'s, but hold out for more than a ¼ per cent premium for lost marketability. These will liquidate their holdings for cash or short terms, and await future opportunities for more profitable long-term investment.

Alternative (1) will freeze existing debt effectively against monetization of debt through sale to the Federal Reserve System. Alternative (2), if justified by events, leaves the door open for monetizing the debt in the *future*. Alternative (3) means increased monetization in the *present*. Unless alternative (1) is taken eventually by holders of the bulk of the existing debt, no great good will have been accomplished by this Treasury gadget, beyond making future issues nonmarketable and less explosive. If the analysis in the text is correct, alternative (1) will be accepted by relatively few bondholders, and relatively little good will have been done.

[13] Lawrence H. Seltzer, "The Problem of Our Excessive Banking Reserves," *Journal of the American Statistical Association*, XXXV (March, 1940), 32–35.

[14] Board of Governors, *1945 Annual Report*, p. 8; "Proposal for a Special Reserve Requirement against the Demand and Time Deposits of Banks," *Federal Reserve Bulletin*, XXXIII (December 1947), 1461–62; *ibid.*, XXXIV (January, 1948), 14–23; S. E. Leland, "The Government, the Banks, and the National Debt," *Journal of Finance*, I (August, 1946), 23–24 (originally published in *Commercial and Financial Chronicle*, January 17, 1946). For a survey of the later literature, together with critical comments, see Edward C. Simmons, "Secondary Reserve Requirements for Commercial Banks," *American Economic Review*, XLI (March, 1951), 123–38.

are no exception. Bankers will object with especial vehemence because these controls will impair their competitive position vis-à-vis such rivals as building and loan associations, public lending agencies, and insurance companies. Objections will also come from small businessmen and to a less extent from farmers. They are peculiarly dependent on commercial banks for working capital, and restrictions on bank credit expansion will impress them as discriminatory. Objections, finally, will come from the increasing body of citizens with "vested interests in inflation," who have speculated on continued inflation to the extent that they stand to suffer financial losses not only from price declines but from price stability. In considering these objections, I shall try to distinguish between those applicable peculiarly to the proposal made here, those applicable to any anti-inflationary program operating from the monetary side, and those applicable to any anti-inflationary program of whatever sort.

VI

Banks exist, and earn their profits, by serving their clients and their depositors. It naturally seems contrary to sound banking policy to restrict banks' services to their most important clients (their borrowers), except in the interest of their depositors. Sound banking practice, it has always appeared, involves only such limitations on bank loans as are required to protect bank depositors. One such limitation is the requirement of adequate reserves against deposits. Another is the unwritten requirement that borrowers have adequate "capital, capacity, and character" to insure the bank, and through the bank its depositors, against avoidable defaults on loans. Bankers tend to see their depositors as "the public," and "the public" as their depositors. Their duty to the public is therefore fulfilled when their depositors' money is safe. In the 1950–51 situation there seems no danger of bank runs such as would require increased reserves or of defaults on loans made with the ordinary degree of caution. What then is the need for further regulation?

The answer can only be given in terms of the inflationary effect of bank-loan expansion, and this answer finds two usual rebuttals in practical banking circles. One is that bankers cannot "create credit," but can only lend out funds with which depositors have intrusted them. This comes close to the truth for the individual bank but is wrong for the commercial banking system as a whole, as can be deduced from our brief analysis of "derivative deposits" earlier in this essay.[15]

Another conventional view among practical bankers denies that loans for "productive" purposes can possibly be inflationary. A productive loan induces an equivalent additional output. Loan expansion, in this view, may be necessitated by higher prices, but it can never cause them, unless the loans are made for "speculative" rather than "productive" ventures.

This is the "productive credit," "real bills," or "needs of trade" theory of banking, which has been dominant for at least two centuries. It was perhaps strongest in Great Britain and America during the half-century immediately prior to World War I and has since fallen

[15] Cf. above, p. 422. Loans made by Bank X become, for the most part, deposits in Banks Y, Z, and so on. When Banks Y and Z expand their own loans on the basis of increased deposits, they need not inquire into the source of the increases and are unaware that credit is being "created" in any sense. Looking at the three banks taken together, however, the possibility of multiple expansion can be seen.

into disrepute in academic circles.[16] Without marshaling all the possible arguments against this plausible and "practical" theory, let us be content with pointing out a crucial limitation under conditions of full employment. When resources are employed fully, any single borrower can of course expand his own output by using his loan for productive purposes. But this is done almost entirely by employing "productive credit" to hire labor and materials away from other bidders. And how can this be done? In the absence of unemployment, it is done normally by raising their prices. To the extent that the prices of labor and materials rise, real output as a whole is not expanded when bank credit is. Borrowers can expand their output, but at the expense of nonborrowers. The money value of output rises with the price level. Only this, and nothing more.

Regulation of bank loans is peculiarly obnoxious to the commercial bankers for the reason that rival lending agencies are left unaffected. Insurance companies and government corporations, in particular, would be untouched by the loan-ratio ceiling. With bank lending restricted, insurance companies would be able to skim the cream of the lending business, financing their expansion if necessary by selling the government securities which they now hold.

This is a valid competitive objection. If banks are to be protected against privileged competition, it may be necessary to let marketable bond prices fall further than otherwise, to prevent insurance companies from expanding over areas vacated by the banks. On the other hand, loans from nonbankers are somewhat less inflationary than bank loans. Insurance loans, security issues, and trade credit do not increase the total of deposit currency as bank loans do. They merely transfer existing deposits from lenders to borrowers. The turnover of these deposits against goods and services may be accelerated, but only to a limited extent. Limits are set by the financial needs of the lenders themselves, which eventually require them to borrow from their rivals (the commercial banks), or else reduce the scale of their own lending activity. It is the existence of such limits which makes their credit less inflationary than that extended by the banks.

Control over bank lending, however, will admittedly increase the short-circuiting of the commercial banking system by all of the devices mentioned in the last paragraph. It will probably increase the velocity of circulation of money. If this velocity rises substantially before the limits on nonbank credit expansion are reached, upward pressure on prices will continue, quite as though the quantity of money had increased. But with bank credit tight, credit from rival sources must tighten as well. Nonbankers can be liberal with business loans, book credits, or security purchases while they are sure of help from their banks if their own position becomes illiquid. But if they cannot expect such help regardless of their technical credit ratings, their attitudes will hardly remain unaffected. If this analysis is correct, nonbank credit too will tighten almost at once, following

[16] The leading contemporary critique of this theory is L. W. Mints, *History of Banking Theory* (Chicago: University of Chicago Press, 1945), devoted largely to tracing its various historical manifestations and attacking each in turn. Among earlier refutations, the standard one remains the 150-year-old classic *Enquiry into the Nature and Effects of the Paper Credit of Great Britain*, published by Henry Thornton in 1802! Little is said now in opposition to the "needs of trade" doctrine which cannot be found in Thornton's essay, but repetition seems to be required in each period of inflation and to each generation of practical bankers. Among twentieth-century works, special attention is perhaps due to the refutation in Dennis H. Robertson, *Money* (rev. ed.; London: Pitman, 1948), pp. 105–7, 182.

a ceiling on the loan ratio, by a half-psychological, half-economic contagion. Upward price pressure will ease at the same time. It may even be replaced by an over-all "money shortage." To the extent that nonbank credit replaces bank credit, upward pressure from increased velocity can be counteracted by further reduction of the loan-ratio ceilings, although this action penalizes the commercial banks for the activities of their competitors.

There exists an unfortunate tendency in banking publications to treat any proposal for additional regulation of banking in the interest of inflation control as a form of nationalization or bankruptcy of the commercial banking system, or a plot to blame private bankers for the financial excesses of the federal government. This reaction was marked particularly in connection with the "secondary reserve" proposal of the board of governors.[17] Talk of bank nationalization through loan-ratio control, however, is pure propaganda in the most derogatory sense of the term. There is nothing closer to bank nationalization in a ceiling on the loan ratio than in the floor on the ordinary reserve ratio. A floor on the reserve ratio has been a feature of bank regulation for over a century.

The effect of a ceiling loan ratio upon bank earnings is by no means clear. Certainly, as the banks are forced to curtail the volume of their loans, or as a rise is choked off which would have occurred otherwise, there will be a tendency for interest rates to successful borrowers to rise. The rise in interest rates will be one method of rationing the reduced volume of credit available relative to the amount demanded. Some banks, however, will prefer to ration credit more directly—by imposing higher standards, rejecting new customers, or what not—while leaving their nominal interest charges unchanged. Whether the over-all rise in interest rates will be sufficient to compensate the banks for the reduction in loan volume cannot be forecasted accurately. Were it not for insurance company and other competition, I might hazard a guess that bank earnings would actually increase, due to the inelastic character of the demand for loans.[18] At any rate, some rise in interest rates to private borrowers can be expected, even though the rate on government securities be held constant by Federal Reserve support of their prices. The market for government securities will thus be isolated further from the ordinary or private money markets.

Any credit restriction policy—not merely the proposal under discussion here—affects small business first and probably hardest. This is particularly true for a strong program, with actual cut-backs and roll-backs involved. Small business is the marginal borrower, whose credit line the banks will reduce first, when over-all restrictions are imposed. While larger companies have frequent access to internal financing, insurance companies, and the securities markets, these are for practical purposes barred to their smaller competitors. Book and trade credit, while available to large and small alike, usually involves higher interest charges to the small borrower. And, finally, the bank credit remaining available to small business will probably be available only at higher interest rates.

It is not at all inconceivable, rather in

[17] See Simmons, *op. cit.*, p. 127, n. 8.

[18] Earnings in small city, suburban, and country banks are most likely to rise, since borrowers from these banks have limited access to other sources of capital. Earnings in large city banks are less likely to rise, as any increase in bank interest rates drives large and well-established borrowers to insurance companies and security flotations.

fact quite likely, that credit cut-backs will cause acute financial embarrassment to small companies who have financed growth by getting into extended financial positions. Many will be forced to dispose of inventories or to reduce the scale of their operations. Some may be forced into liquidation or even bankruptcy.

The small businessman (and farmer) is then the principal victim of monetary stringency. The imposition of monetary controls will probably tend to increase the concentration of American business among the larger concerns. The validity of this objection must be granted, yet the fixed-income consumer and other victims of inflation can hardly have helped observing that these small businessmen and farmers have done well enough for themselves while prices were rising to have lost any claim on our lachrymose sympathy when controls are being invoked. But this is an ethical judgment rather than an economic one.

A particular group of small businesses presenting special problems are defense plants, chiefly of the subcontracting variety, which rely on local banks for their working capital. They may not be able to obtain such capital, if these local banks are kept from lending by ceilings on their loan ratios. It will be easy to cite their difficulties as representing interference of "finance" with the defense effort and to make a case in the name of patriotism for doing away with the obstruction.

In reality, it is not "finance" but full employment which will be halting defense production. It is not basically money which is unavailable when bank credit is reduced but the real goods and services which the money represents. Supplying labor and materials to defense plants requires taking labor and materials away from others. Any alleged shortage of credit for defense plants means that banks are unable or unwilling to take labor and materials away from other companies by restricting their credit hard enough or fast enough.

Defense contractors and subcontractors who cannot be accommodated by the banks should be able to obtain credit directly from a centralized source. The Reconstruction Finance Corporation, currently under a cloud because of charges of corruption in its operations, is such a source. The Federal Reserve banks are other possible sources. They set up V-Loan and T-Loan programs to assist in defense plant financing during and after World War II. Enforcement of loan-ratio ceilings against commercial banks would require their operation on a much larger scale in the present situation, particularly if extreme conservatism is forced on the Reconstruction Finance Corporation.

The volume of commercial loans to defense plants financed through the RFC and Federal Reserve System should naturally be considered in setting loan-ratio ceilings. These loans result in deposits which are "original" to the commercial banking system and which are if anything more inflationary than ordinary commercial loans. The more of such extraordinary federal financing is required, the lower should the loan-ratio ceiling be set for the commercial banks, and the more rapidly should it be reduced.

Whenever an inflationary movement is halted abruptly, or a price roll-back is attempted on a general scale, there is real danger of setting off waves of depression and unemployment. With many prices and wages rigid against downward movement, profit margins are squeezed, sometimes to the vanishing point or below. Consumer psychology also may veer toward "elastic expectations" in the downward direction, with each price cut lead-

ing to the postponement of purchases in anticipation of further cuts to come. Cumulative downward movements may even lead conceivably to "hyper-deflation," such as occurred in 1931–33.

It should nevertheless be easy to avoid such excesses while adhering to a firmly deflationary policy. This is most likely if the monetary authorities, in this case the Board of Governors of the Federal Reserve System, state with some authority and in advance the goal of their actions in quantitative terms. They are not seeking, they should make clear, to bring prices down, but down to some specified level, such as that of January, 1950, or 25 per cent above the level of V-J Day, 1945; and they should declare that they firmly intend to maintain this level against downward as well as against upward pressure. Such a clear statement of policy, especially if put in price-level terms, will be of real value in avoiding anticipations of cumulative deflation. It will also make less dangerous the inevitable time lags in the price effects of changes in the loan-ratio ceiling.

The problem of rigidities, of the refusal of particular prices and wage rates to budge from their inflationary peaks, is more serious. It has caused the great bulk of the transitional loss in output and employment which has followed every reversal of major inflationary movements. It will almost inevitably cause considerable loss of output and employment, at least temporarily, if this proposal is put into effect, or if any other efficient monetary or fiscal measures for inflation control are enforced. Chairman Leon Keyserling of the Council of Economic Advisers is perhaps the most prominent of many American economists who, measuring prosperity by the volume of production and impressed with the desirability of growth of production at maximum speed, have tended to exaggerate the cost of inflation control in temporary slackening or reversal of growth and to ignore the distributive cost of inflation itself. This position is analogous to that of the parents of kidnapped children, who pay ransoms in preference to calling the police and risking injury to their loved ones. It is a wrong decision from the viewpoint of social ethics, and perhaps from long-run self-interest as well; and yet who can condemn his fellows for yielding to it?

The beneficiaries of high and rigid wages and prices, which will become unbalanced with the remainder of the economy when the general price level ceases rising, are among the groups with the strongest vested interest in continued inflation. The problem of rigidity in prices and wage rates is merely one important part of the general problem of "vested interest in inflation" to which we now turn.

VII

The vested interest in inflation is already stronger in present-day America than it ever became during World War II. The prevailing belief at that time was based upon 1919–21 experience and upon the forecasts of postwar depression. It was thought at the time that inflation must lead to depression, that "what goes up must come down." The events of 1946–48 have shaken this psychological set. The present belief appears rather that "what goes up must rise further," and more and more people pay higher and higher prices for "hedges" against future price increases.

Who has a vested interest in inflation? Everyone who stands to suffer financial loss not only from price deflation but from price stability. More specifically,

there are three major groups in whom such an interest has become vested.

1. Holders of real estate, securities, and inventories who have made their purchases at prices which include a premium for "inflation hedge." They stand to lose not only if general prices fall but if they stay constant, for under constant general prices the "hedgehogs" drop out of the demand side of the market for the "hedge" goods, and the "hedge" goods themselves fall in price. Building contractors and materials men, and other producers of "hedge" goods, are indirectly in the same category.

2. Then there are wage-earners whose hourly or weekly rates are so high that only the hoarding and pirating of labor, or production for inflation hedge purposes, assures them of steady employment. They stand to take cuts in employment, wage rates, or both (probably both), if the general price level is stabilized. These cuts will of course be greater and last longer if prices fall.

3. Finally, there is the growing body of persons with fixed money debts, whose current incomes are more or less protected by "escalator clauses" in one form or another. Their disposable incomes, after payments on their fixed debts, rise with the price level. The homeowner with an escalator clause in his wage contract and a mortgage on his home may have more to spend on current consumption, in real terms, the higher the price level rises. Such people do not normally realize their stake in maintaining the value of money. The "money illusion" may be lifted for them as regards present money incomes, but for future money incomes it remains in control. These people do not realize what is happening in the present to the future purchasing power of their fixed money credits—pensions, annuities, or insurance. Or perhaps they expect eventual "escalation" to cover these items as well as current ones. This sort of thinking, or nonthinking, is an example in monetary economics of that "underestimation of the future" which Böhm-Bawerk considered basic to human economic psychology.

Vested interests in inflation apply against any effective method of inflation control, whether direct or indirect. They constitute the "real reasons" back of some of the "good reasons" outlined above against the loan-ratio ceiling proposal. Also the strongest reasons. They operate not only against this proposal but against other monetary and fiscal plans for inflation control. They operate against direct controls as well—as Messrs. Johnston, Wilson, and DiSalle can doubtless testify.

Vested interests in inflation must simply be overcome by frontal attack, if the value of the dollar is to be maintained. The "hedgehogs" among us will have to sacrifice for the benefit of the remainder. Temporary losses in disturbed expectations, industrial conflict, reduced production, and unemployment will affect a larger number. Either we possess the courage required to overcome these vested interests within the framework of our traditional institutions, or we do not. If we have courage, well and good. If not, we may yet, for all our vaunted productive capacity, follow the road to a barter economy or to whatever measures of dictation or confiscation may be required to prop up the purchasing power of our money.

ARE PROPERTY TAX RATES INCREASING?[1]

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I

IT IS generally believed that, although property tax rates have been rising, local governments in the United States no longer have property tax resources necessary to support their expanding public services.[2] This paper examines the available data on true property tax rates in the United States from 1922 to 1949 in order to test the belief that localities have been taxing property more heavily.

The true property tax rate is the ratio of the property tax to the market value of the taxed property. This rate differs from the official tax rate which is based on assessed valuations. Since the ratio of the assessed to the market value of property is not uniform in different areas and changes frequently in the same locality, one must examine true rates in order to avoid the effects of variations in assessment ratios.

Two principal sources of statistics on true property tax rates are available. The Bureau of Agricultural Economics has made estimates for farm real estate since 1909.[3] The Bureau of Governmental Research has compiled such information for property in urban areas since 1922.[4] The significance of these estimates is indicated by the large proportion of total property tax revenue they represent. In 1930 the 310 cities having a population of over 30,000 accounted for 53.5 per cent of the total. From 1924 to 1948, taxes levied on farm real estate ranged from 8.8 per cent to 13.7 per cent of the annual revenue from property taxation.

[1] I am deeply indebted to Professor Richard Goode for helpful suggestions and criticisms.

[2] Professor Groves writes: "At the local level, the general property tax has proved itself as inadequate and as unsuitable to finance the needs of a modern city as it did forty years ago to finance the states" (Harold M. Groves, "A General Appraisal of the American Tax Problem," *Annals of the American Academy of Political and Social Science*, CCLXVI [November, 1949], 51). A study by the Federal Reserve System states that localities "suffer the disadvantages of a tax system inadequate to meet steadily increasing demands for public services" (Board of Governors of the Federal Reserve System, *Public Finance and Full Employment* ["Postwar Economic Studies," No. 3 (Washington, 1945)], p. 105). Professor Somers writes that property taxation "has been exploited more and more as needs have grown. The trend continues" (H. M. Somers, *Public Finance and National Income* [Philadelphia: Blakiston Co., 1949], p. 431). On the other hand, in his history of property taxation in the United States, Professor Jensen wrote: "Is there a marked tendency toward increased property taxes, when expressed correctly as a percentage of the true value of the taxed property? Unfortunately, no study has been made to show whether taxes actually are rising as fast as complaints would lead one to believe" (Jens P. Jensen, *Property Taxation in the United States* [Chicago: University of Chicago Press, 1931], pp. 86–87).

[3] The Bureau of Agricultural Economics of the United States Department of Agriculture has made estimates of average tax levies on an acre of farm real estate per $100 of value for each of the forty-eight states, nine geographical divisions, and the United States as a whole. The bureau states that its computations are based solely on real estate.

[4] The estimates by the Bureau of Governmental Research, a private agency located in Detroit, are the average true rates on property in urban areas usually consisting of several units of local government. The bureau does not explicitly exclude personal property, but it is probable that the assessment ratios used to compute true rates are based primarily on real estate. The bureau bases its estimates on official property tax rates and does not take into consideration the possible difference between levies and collections.

II

Table 1 shows the trend of true property tax rates on farm real estate. Table 2 shows the trend in cities having populations of more than 30,000 and 250,000, respectively. In the larger cities, the countercyclical fluctuations of average true rates are more pronounced. This probably results from more accurate assessment ratios.

The method of the Bureau of Agricultural Economics is superior to that of the Bureau of Governmental Research. The former computes true rates by obtaining the ratio of average tax levies per acre to the average value of an acre of farm

TABLE 1*

TAX LEVIES IN THE UNITED STATES ON FARM REAL ESTATE PER $100 OF VALUE, 1922–49

Year	Taxes per $100 of Value
1922	$0.96
1923	1.01
1924	1.03
1925	1.07
1926	1.12
1927	1.15
1928	1.18
1929	1.19
1930	1.30
1931	1.44
1932	1.54
1933	1.28
1934	1.19
1935	1.15
1936	1.16
1937	1.19
1938	1.19
1939	1.23
1940	1.22
1941	1.18
1942	1.08
1943	0.95
1944	0.91
1945	0.90
1946	0.90
1947	0.96
1948	1.00
1949	1.10

* Source: *Agricultural Finance Review*, XII (November, 1949), 104, and Department of Agriculture, *Taxes Levied on Farm Real Estate in 1949* (Washington: Bureau of Agricultural Economics, August, 1950), p. 2.

TABLE 2*

TREND OF TRUE PROPERTY TAX RATES IN LARGE CITIES OF THE UNITED STATES, 1922–49

Year	Cities Having a Population over 30,000		Cities Having a Population over 250,000 in 1930	
	No. of Cities†	True Property Tax Rates per $1,000 of Value‡	No. of Cities	Arithmetic Average Rate per $1,000 of True Value§
1922	29	$24.17	26	$24.90
1923	29	24.17	33	23.59
1924	131	23.51	34	23.48
1925	156	23.29	36	22.46
1926	167	23.22	33	22.55
1927	186	23.45	34	22.82
1928	199	23.64	33	22.96
1929	191	23.75	33	23.01
1930	144	23.76	32	23.29
1931	162	24.15	37	24.34
1932	241	25.00	37	24.58
1933	241	24.43	37	25.20
1934	217	25.14	36	25.39
1935	233	24.95	37	25.57
1936	230	25.56	37	26.33
1937	225	26.38	37	26.52
1938	248	27.03	37	27.17
1939	257	27.48	36	27.61
1940	252	27.77	37	28.02
1941	263	27.79	37	28.17
1942	271	27.74	37	28.05
1943	290	27.39	36	28.36
1944	249	27.51	37	27.95
1945	250	27.73	37	27.91
1946	249	28.25	37	29.62
1947	301	28.20	37	27.72
1948	303	29.93	37	28.79
1949	248	30.11	35	27.91

* Source: The annual reports by the Bureau of Governmental Research, Detroit, in the *National Municipal Review*, Vols. XI–XXXIX (1922–50).

† The number of cities for which rates are available in two successive years.

‡ The true rates are based on (1) the average rate in 1922 and (2) the amounts of change in the arithmetic average of true rates for cities reporting in both of two successive years.

§ When data for some of the 37 cities are lacking, the rate computed is the average of those cities for which estimates are available.

real estate.[5] Data on taxes per acre are primarily from official tax records. Data on the value of farm real estate are from owners' estimates compiled by the Bureau of the Census and from the index of farm land values computed by the Bureau of Agricultural Economics. The latter has been based primarily on estimates by crop reporters of the average value of all farm land (including improvements) in their localities. Both the tax-per-acre and value-per-acre figures are weighted according to the area of land in farms.

The Bureau of Governmental Research computes true rates in cities by multiplying official rates by estimates of the average ratio of the assessed to the true value of property.[6] The bureau obtained its data from questionnaires sent to city and county officials, bureaus of municipal research, and chambers of commerce. Estimating accurate assessment ratios for property in cities has been difficult.[7] The true property tax rate for groups of large cities computed by the Bureau of Governmental Research is a simple arithmetic rather than a weighted average.

[5] For a description of the method used by the Bureau of Agricultural Economics see Donald Jackson and G. J. Isaac, *Farm-Property Taxes and Their Relation to Parity Determinations* (Washington: Department of Agriculture, 1941), pp. 13–24, and Department of Agriculture, *Agricultural Estimating and Reporting Services of the U.S. Department of Agriculture* (Washington: Government Printing Office, 1949), pp. 161–62.

[6] For an explanation of the method used by the Bureau of Governmental Research see C. E. Rightor, "A Commentary upon the Comparative Tax Rates of Thirty-two Cities, 1922," *National Municipal Review*, XI (1922), 412, and Rosina K. Mohaupt, "Comparative Tax Rates of 244 Cities, 1938," *National Municipal Review*, XXVII (1938), 596.

[7] For a discussion of some of the problems encountered in attempts to estimate assessment ratios from sales data see Illinois Tax Commission, *Survey of Local Finance in Illinois*, Vol. VIII: *Tax-Rate Limits and Assessment Ratios, 1925–1940* (Springfield, 1940).

III

One would expect the trend of true rates for farm real estate to differ occasionally from that for urban property. The differences in the tabulated trends may also have resulted from the use of incorrect data. Tables 1 and 2 can be roughly examined by comparing true rates in 1922, 1929, 1934, 1940, and 1949 —years that encompass periods of price stability, deflation, and inflation.

From 1922 to 1929, true rates on farm real estate increased 24 per cent. At the same time, true rates fell 1.7 per cent in cities with a population over 30,000 and 7.6 per cent in the thirty-seven cities with a population over 250,000. This difference between the trends for farm and urban real estate resulted primarily from opposite movements in the total market values of these types of property while in both farm and urban localities the need for revenue was increasing. The total market value of urban real estate increased from $122.2 billion to $266.3 billion between 1922 and 1930, the total market value of farm real estate declined from $54.2 billion to $47.9 billion.[8] Naturally, the need for revenue in farm and urban localities did not vary in direct proportion to the changes in the total market value of property.

True rates on farm real estate were the same in 1934 as in 1929, even though they had risen 29 per cent during the first three years of this period. On the other hand, in 1934 true rates were 5.9 per cent higher in cities with a population over 30,000 and 10.4 per cent higher in the more populous cities. During the depression tax levies were more flexible on farm property than on urban real

[8] D. L. Wickens, *Residential Real Estate* (New York: National Bureau of Economic Research, 1941), p. 2.

estate.[9] Since property tax levies depend upon government expenditures, this may have resulted from the more flexible expenditures in rural areas. Another explanation is that the reduction in farm tax levies in 1932–34 was an attempt to lower rates that had been climbing even during the 1920's. Since true rates on urban real estate did not increase before the depression, a later adjustment of urban rates would be expected.

Data in Table 2 do not show accurately the extent of the rise in true rates on urban property between 1929 and 1934. Many of the assessment ratios used by the Bureau of Governmental Research failed to increase when the market prices of real estate fell.[10] When market values of property fall more rapidly than assessed values, assessment ratios should rise. Although the total assessed value of property in thirty-two of the cities with a population over 250,000 fell 16.3 per cent during these years, the average drop in urban real estate values was probably as much as one-third.[11]

From 1934 to 1940, true rates increased almost 10 per cent for both groups of cities, but only 2.5 per cent for farm real estate. This estimate of the rise in urban rates was probably too large. It resulted from higher official tax rates combined with the use of assessment ratios that failed to decline when property values rose slightly.

True rates for farm real estate fell 9.8 per cent from 1940 to 1949, but they went up 8 per cent in cities having a population over 30,000. A small reduction of 0.4 per cent occurred in the thirty-seven largest cities. Census data on property tax revenue in cities do not support this trend of urban rates during the 1940's. From 1942 to 1948 total property tax revenue in the 397 cities with a population over 25,000 increased only 22.1 per cent.[12] During these years market values of urban real estate undoubtedly increased more than property tax revenue. Therefore, true rates in large cities must have fallen.

The reason why the data of the Bureau of Governmental Research do not show the actual decline in true rates on urban property is that many of the assessment ratios it used did not decrease when market values of property doubled or even tripled.[13] From 1940 to 1949,

[9] The estimates by the Bureau of Agricultural Economics show that tax levies per acre on farm real estate decreased 36.2 per cent from 1929 to 1934 (*Agricultural Finance Review*, XII [November, 1949], 104). Total property tax revenue in the United States declined only 14.6 per cent from 1929 to 1934 (Treasury of the United States, *Federal, State and Local Government Fiscal Relations* [Senate Doc. No 69 (Washington: Government Printing Office, June, 1943)], p. 352).

[10] An examination of the assessment ratios for the thirty-seven cities with a population over 250,000 shows that in eleven cities the assessment ratio used in 1934 was less than the ratio used in 1929, in four cities there was no change, and in only sixteen cities was the assessment ratio higher in 1934. For six cities comparable assessment ratios were not available. In those cities in which the assessment ratio was higher in 1934, the increase in the assessment ratio was small if one considers the fall in the market prices of property during these years.

[11] The estimated decrease in assessed valuations is based on data reported by the Bureau of Governmental Research for 1929 and 1934 in the *National Municipal Review*. Wickens (*op. cit.*, p. 2) has estimated that the total value of nonfarm real estate declined 34 per cent from 1930 to 1934. The total assessed value of property includes personal property as well as real estate. The value of personal property may not have decreased as much as the value of real estate. However, real estate probably comprises more than 80 per cent of taxed property in large cities (Bureau of the Census, *Property Taxation in 1941* ["State and Local Government Special Study," No. 22 (Washington, September, 1942)], p. 41).

[12] Bureau of the Census, *Compendium of City Government Finances in 1948* (Washington: Government Printing Office, 1950), p. 5.

[13] An examination of the assessment ratios for cities having a population over 250,000 shows that in seven cities the assessment ratio used by the bu-

total assessed valuations in the thirty-seven largest cities increased only 36 per cent, so that assessment ratios should have decreased.[14] Another indication of inaccuracy is that the assessment ratios used by the bureau changed abruptly. Hence the estimates of true rates prior to the changes were probably incorrect.

When such deficiencies in the data collected by the Bureau of Governmental Research are taken into consideration, its estimates and those of the Bureau of Agricultural Economics fail to support the common belief that property tax resources are being taxed more heavily. During the 1920's, although true rates on farm property went up 24 per cent, the rates on urban real estate decreased slightly. During the first two or three years of the depression, true rates increased sharply; however, in 1932–34 true rates on farm property fell. From 1934 to 1940 the rise in rates was small. By 1941–42 true rates started down, and at the end of World War II they had fallen to the level of the 1920's. For farm real estate the average true rate in 1947 was the same as in 1922. True rates on farm property started to rise in 1947 and continued upward in 1948–49.

IV

Data on total property tax revenue in the United States, new private construction, and real estate prices provide a rough method of checking the trends estimated by the Bureau of Governmental Research and the Bureau of Agricultural Economics. The available indexes of construction costs will supplement the limited information on real estate prices. The trend of true property tax rates in the United States can be estimated by comparing changes in total property tax revenue with changes in the base of the property tax resulting from new construction and variations in the prices of real estate.

From 1922 to 1929 the large amount of new construction tended to maintain true rates at approximately the same level even though local governments expanded levies. Estimates by the Bureau of Labor Statistics and the Office of Domestic Commerce indicate that the total value of new private construction in 1922–28 was $54.6 billion.[15] At an official tax rate of $30 per thousand,[16] this addition to the tax base would add $1,638 million annually to total property tax revenue.[17] This revenue in the United States actually increased $1,548.7 million during these years.[18] Although prices of urban real estate probably went up during the 1920's, new construction was the main factor affecting the base of the property tax.

reau in 1949 was the same as the ratio in 1940, in nine cities the assessment ratio was larger in 1949, and in twenty cities the assessment ratio was less. Data for both years were not available for one city.

[14] This estimate is based on the assessed valuations of property in the cities as reported by the Bureau of Governmental Research for 1940 and 1949 in the *National Municipal Review*.

[15] United States Housing and Home Finance Agency, *Housing Statistics*, III (January, 1950), 10.

[16] Data on official property tax rates compiled by the Bureau of Governmental Research indicate that average official tax rates in cities varied from $28.29 per $1,000 in 1922 to $32.02 per $1,000 in 1929.

[17] The estimates by Kuznets of new capital formation exclusive of government capital formation from 1922 through 1928 amount to $43 billion (Simon Kuznets, *Commodity Flow and Capital Formation*, I [New York: National Bureau of Economic Research, 1938], 494). At an official tax rate of $30 per thousand, this net capital formation would add $1,290 million to annual property tax revenue.

[18] The data on total property tax revenue in the United States from 1922 to 1949 are from the studies of state and local government finance by the Bureau of the Census and from a study by the Treasury of the United States entitled *Federal, State and Local Government Fiscal Relations*.

From 1929 to 1934 the decline of approximately one-third in real estate values sharply contracted the property tax base. The addition to taxable property because of new construction in 1929–30 was probably offset by net depreciation of property in 1931–33. During this period property tax revenue dropped 14.6 per cent. If the average true property tax rate was 2 per cent in 1929,[19] decreases of 34 per cent in the total market value of taxable property and 14.6 per cent in total property tax revenue would increase true rates 29 per cent.

A small advance in real estate prices affected the amount of taxable property from 1934 to 1940. Again, new construction may have expanded the tax base in some years, but in other years there was net depreciation. The composite index of construction cost compiled by the Department of Commerce went up 8.8 per cent during these years.[20] This is strikingly similar to the 10.5 per cent rise in the index of farm real estate values estimated by the Bureau of Agricultural Economics.[21] Total property tax revenue in the United States was only 2 per cent higher in 1940. Under these conditions, if the average true rate was 2 per cent, an 8 per cent increase in real estate values would reduce true rates by 5.5 per cent.

From 1940 to 1949 higher real estate values significantly affected the base of the property tax. The composite index of construction cost went up 103.8 per cent from 1940 to 1948. The large amount of construction after World War II was offset to some extent by restrictions on private construction during the war. Total property tax revenue increased 44.8 per cent. If the average true rate was 2 per cent in 1940, these increases in the market value of property and in total property tax revenue would lower true rates 27.5 per cent.

When compared with the trend of total property tax revenue in the United States, information regarding new construction and the prices of taxed property indicates that true rates remained the same from 1922 to 1929, increased 29 per cent from 1929 to 1934, fell 5.5 per cent from 1934 to 1940, and decreased 27.5 per cent from 1940 to 1948.

V

The trend of true property tax rates shown by the statistical data has largely resulted from several developments in state and local government finance. One of these is that property taxation has become less important as a source of revenue for local governments. Because localities have tapped other sources of revenue, property tax levies have tended to be smaller and thus true rates to be lower. Although property taxes did not diminish in importance before 1932, they fell as a percentage of total local government revenue from 72.5 to 52.7 between 1932 and 1949.[22] If local governments in 1949 had obtained 72.5 per cent of their general revenue from this source, total property tax revenues of local govern-

[19] From 1922 to 1949 true property tax rates ranged from approximately 1 per cent to 3 per cent. Variations of 1 per cent in the assumed true rate have an insignificant effect on the estimated change in true property tax rates.

[20] United States Housing and Home Finance Agency, *Housing Statistics*, III (January, 1950), 12.

[21] A. R. Johnson, *The Farm Real Estate Situation, 1946–47* (Department of Agriculture Circ. No. 780 [Washington: Government Printing Office, March, 1948]), pp. 4–5.

[22] Bureau of the Census, *Historical Review of State and Local Government Finances* ("State and Local Government Special Studies," No. 25 [Washington, June, 1948]), p. 13; Bureau of the Census, *Governmental Revenue in 1949* ("Governmental Finances in the United States: 1949," No. 2 [Washington, August, 1950]), p. 9.

ments would have amounted to approximately $9 billion instead of the $6.5 billion collected. The adoption of state laws limiting local property tax rates has encouraged the shift to other sources of revenue. Five of the nine states with over-all tax limits adopted such laws during the depression years from 1929 to 1936.

State governments also have relied less and less on property taxation. On the average, states obtained 27.6 per cent of their total general revenue from this source in 1923 and only 2.5 per cent in 1949.[23] Although state revenue from property taxation for the entire period has been only a small percentage of the total, its declining importance has tended to reduce the amount of property tax revenue and thus to lower true rates.

The trend of local government expenditures has also affected true property tax rates. Total money expenditures of local governments increased 63 per cent from 1923 to 1932, remained constant from 1932 to 1945, and rose 100 per cent from 1945 to 1949.[24] During the 1920's larger money expenditures of localities tended to raise property tax levies and, if the value of the tax base had remained constant, would have increased true rates proportionately. From 1932 until the end of World War II, the stability of local government expenditures tended to keep levies and true rates the same. Since World War II, regardless of the trend of real local government services, the larger money expenditures again would have increased true rates if the value of the tax base had not risen.

There is no indication that rates of tax delinquency changed from the 1920's to the 1940's, but they did affect true rates during and following the depression.[25] Widespread delinquency during the depression tended to lower average true rates by reducing the property tax revenue collected. After the contraction phase, the payment of delinquent taxes had the opposite effect. Larger collections tended to increase average true rates.

During the 1930's the adoption of homestead exemption laws in some states raised true rates on taxable property by shrinking the tax base. Homestead exemptions gained headway during the 1930's, but there were few changes in these laws in the 1940's. At the present time thirteen states either exclude homesteads or grant them preference in taxation.

VI

True property tax rates tend to rise during depressions and to fall during periods of prosperity because tax levies tend to lag behind changes in the market values of property. A greater decline in property values than in levies increased true rates during the depression. Since property values advanced more rapidly than property tax levies in 1940–49, true rates fell.

Several reasons may be cited why tax levies lag behind real estate values. Property values change promptly with

[23] Bureau of the Census, *Historical Review of State and Local Government Finances*, pp. 19–20; Bureau of the Census, *Summary of State Government Finances in 1949* ("State Finances: 1949," No. 1 [Washington, May, 1950]), p. 5.

[24] Tax Foundation, *Facts and Figures on Government Finance, 1950–1951* (New York, 1950), pp. 52 and 89. Total money expenditures of local governments include expenditures financed by grants-in-aid but do not include debt retirement.

[25] F. L. Bird, *Trend of Tax Delinquency 1930–1940 in Cities of over 50,000 Population* (New York: Dun & Bradstreet, 1941); J. Rosa, "Farm Property Tax Payment and Rural Government Finance," *Agricultural Finance Review*, Vol. IV (May, 1941).

market conditions, while levies vary periodically with administrative changes in assessments or official tax rates. Tax levies also tend to lag because government costs, upon which levies largely depend, may change more slowly than prices in general. In addition, localities are not always able to adjust levies to changing price levels, because total expenditures do not necessarily vary directly with the cost of government services.

Because business cycles have important effects on true property tax rates, some economists have concluded that true rates vary inversely with business fluctuations.[26] Many exceptions to this proposition will be found in the tables if one compares changes from year to year in true rates on farm and urban real estate with changes in net national product or total cash farm income. The countercyclical explanation of the trend of true rates assumes stable tax levies. Actually, local governments have not maintained constant levies regardless of fluctuations in prices and income. In addition, changes in property values may not be perfectly cyclical. Farm real estate values, for example, do not always vary directly with farm income. But, in spite of the exceptions, in the majority of years the trend of true rates has been countercyclical.

Since tax levies tend to lag behind the market value of property during inflation, it is possible that following World War II tax levies had not yet caught up with the rise in real estate values. If so, true rates were relatively low in the last years shown in Tables 1 and 2. This is probably the principal shortcoming of attempts to compare true property tax rates following World War II with rates during the 1920's.

[26] Board of Governors of the Federal Reserve System, *op. cit.*, p. 105.

VII

The statistical data on true property tax rates and the related historical developments are interesting because they discredit the commonly accepted belief about the degree to which localities have been using their property tax resources. Property tax rates, when expressed as a percentage of the true value of property, were not higher in 1949 than in the 1920's. The decline in true rates during the 1940's indicates that localities were using their property tax resources less intensively. Many economists have based their policy suggestions for local governments on the belief that property tax rates have risen. They have recommended local sales and income taxes, expanded grants-in-aid, and the transfer of functions to the state or federal governments as methods of avoiding further increases in property taxes.

During the 1940's rising prices have brought financial difficulties to many local governments. But the inadequacy of revenue relative to expenditures has largely resulted from the failure of localities to raise assessments. As inflation has progressed, expenditures have increased, but assessed valuations have remained approximately the same. In New York City, for example, the total assessed valuation of property was $16.5 billion in 1940 and only $17.6 billion in 1948. In Philadelphia the total assessed value of property was $3.3 billion in 1940 and $3.4 billion in 1948.[27] In many other

[27] The amounts of the total assessed value of property are from the annual reports for 1940 and 1948 by the Bureau of Governmental Research in the *National Municipal Review*.

cities assessed valuations have been virtually unaffected by inflation.

Actually, property taxation is an expansible source of revenue during a period of rising prices. Inflation increases the base of the property tax. This is a desirable characteristic of local government finance because a rise in the general price level is one of the principal causes of larger government expenditures. But the expansion of property tax revenue is not automatic. Local government officials must be able and willing to reassess property at higher levels or to raise official property tax rates.

THE LIFE OF JOHN MAYNARD KEYNES[1]

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IN THE Preface of the awaited Keynes biography, R. F. Harrod explains that he was solicited by Mr. Geoffrey Keynes to undertake the serious task of unveiling the many-sided life of John Maynard Keynes. Harrod had access to Keynes's personal files and the sincere co-operation of the family; thus he can be viewed as the official or definitive biographer. There will undoubtedly be further writings on Keynes's life, but Harrod has fashioned a product that will remain as a memory of an illustrious career.

The Life of John Maynard Keynes is thorough and revealing. Harrod set aside his special pen for political tracts or breezy economic analysis which we have learned to associate with some of his other writings and drew out a pen for recording careful, painstaking thoughts accompanied by great insight. This volume has already made an impression on readers in England and in this country. It is, in my judgment, especially interesting to Americans. It puts us in touch with unfamiliar aspects of Keynes's life and a range of activities in British intellectual circles for almost the first half of this century. It is, indeed, a story of the times. Moreover, an *n*th-hand folklore about Keynes's activities has grown up in this country, perhaps because of the relative remoteness of his locale. One of the great virtues of the *Life* is to clear the record of false items that inevitably get mixed with the truth in the retelling. In this volume we learn, authentically, of Keynes's early associations with classical scholars, his work in the government, the building of his fortune, his journalistic output, his cultural activities, his political activities, and the development of his economic thought.

Many pages of the *Life* are devoted to Keynes's student days at Eton and Cambridge. While there has been some question raised as to the appropriateness of detail on this phase, I find it rewarding as a background for understanding subsequent developments. As an American reader, I find it an interesting description of English student life, although I dare not generalize from Keynes to other students. At the other extreme, Harrod spends many pages on Keynes's work during World War II, Bretton Woods, and the negotiations for the United States loan to Britain. This, too, is interesting material, in that it clears up many details that would otherwise be left obscure for outsiders. We all know, in a rough way, of the "Keynes Plan" for an international clearing union, followed by his participation at Bretton Woods and his activity in negotiating the loan to Britain; but, for the most part, we know little of the magnitude of his actual work along these lines during his last years. Harrod performs a great service in making this information available in detail, although I do feel that he greatly over-

[1] A review note of R. F. Harrod, *The Life of John Maynard Keynes* (New York: Harcourt, Brace & Co., 1951). Pp. xvi+674. $7.50.

stresses the basic importance of this work after the early part of World War II.

The period following World War I and culminating with the publication of the *General Theory* is of particular importance in understanding how the theory of employment grew out of attempts to deal with the current economic situation. Students of doctrinal history have long noted the importance of the bearing of current problems on the development of theory, and Harrod leads the reader through this period so as to leave him with no doubt about the concrete situations which gave rise to the modern theory of employment. Interesting aspects of this developmental period are Keynes's association with the Liberals, Lloyd George in particular; his service on the Macmillan Committee; his excursion into the problems of the depressed cotton industry; his writings in popular journals and the press; and his financial dealings. The story is extremely informative and gives, better than any other attempt, an account of the many interests followed by Keynes in this critical period. The digression on the Lancashire cotton spinners shows the facility with which Keynes hopped from one technical problem to another, while the account of his financial dealings gives us a fuller appreciation of his theoretical work on money, interest, and the securities markets.

The biographer cannot complain of the scarcity of raw material in this fascinating life, but he may find great difficulty in assimilating all the pieces into a coherent whole. To characterize Keynes's enormous span of activities, we should have to label him as economist, university don, civil servant, philosopher, author, editor, journalist, financier, speculator, patron of the arts, book collector, art collector, administrator, and farmer. According to Harrod, he excelled in practically everything he undertook.

What is the composite picture of Keynes the man drawn by Harrod? We see him as a product of British middle-class, intellectual life. He was, from student days on, a great debater, thinking fast on his feet and dealing out verbal destruction to his opponents. He had a taste for intellectual and aesthetic things, especially fine literature, painting, and the ballet. His association with the "Bloomsbury set" and other intellectuals gave evidence of, and influenced, his tastes, to which he referred as the "good life." Harrod looks upon his economics as a scientific choice of means to achieve his end of the "good life."

His early memories of a home fathered by a Cambridge scholar and mothered by an active, intelligent woman are cited by Harrod as basic in understanding Keynes's tastes in life. The Cambridge of Keynes's childhood was "an epitome of England." Harrod refers emphatically to his "presuppositions of Harvey Road," the location of the family home in Cambridge. Keynes apparently thought that a small number of intellectuals really saw matters correctly in the world and that, by their clever schemes, they would enable us eventually to reach the "good life." Harrod says that one of Keynes's presuppositions was that the British government actually lay in the hands of an intellectual aristocracy. The attachment to schemes of monetary management illustrates this point of view. Keynes put forth these schemes on the assumption that improper management was at the root of many economic ills and that the adoption of an intelligent mechanism would mitigate the trouble.

In politics Keynes was a Liberal. During the major part of his career he was an active member of the Liberal party. When he ultimately took a seat in the House of Lords, he officially went as an independent, but expressed a desire to sit

with the Liberals. An important aspect of Keynes's liberalism was that it was not purist; it was more realistic. He was willing to compromise with certain liberal principles, notably free trade, in order to realize his larger objective of the "good society" guided by an intellectual aristocracy. He refused to accept a slavish liberal position that would require him to attack monopoly in any form, place, or time. There can be no doubt, in spite of loose charges by adversaries, that Keynes had no leftist sympathies. Even the Fabians were much too radical for his tastes, and he drew away from association with the *New Statesman* when that journal did not conform to his moderate liberal politics.

The biography is exceedingly idolatrous. Controversies are inevitably resolved in favor of Keynes, and the exposition becomes highly apologetic at places. This is especially true of the controversy associated with the *Economic Consequences of the Peace*. Harrod, as a result of this work, should be known as the world's true Keynesian. He sides with Keynes in economics, art, politics, and almost any other matter involving controversy. The only thing that seems to have shocked Harrod's liberal spirit was Keynes's advocacy of protection during part of the interwar period. Even on this issue Harrod tries to explain his hero's defection but gives the impression that he really frowned on Keynes's policy.

While the *Life* has already been cited as a literary accomplishment of the highest order, Harrod's idolatry has led him to write some "purple" passages that annoyingly mar an otherwise pleasing style. On the occasion of Keynes's placement as second in civil service examinations, necessitating his entry into the India Office instead of the other alternative, the Treasury, Harrod writes:

> Had he gone to the Treasury, he probably would not have come out after two years. Would he as a permanent, and not a temporary, Civil Servant have resigned at Paris in 1919? What would have been the balance of good? Rising towards the top of the Treasury in the inter-war period, would he have achieved a better conduct of British finances? We can hardly doubt that Mr. Churchill, the innocent victim, as Chancellor of the Exchequer, in the crucial years (1925–29), of the old orthodox school at the Treasury and the Bank of England, would have found in Keynes a man after his heart's desire. We may guess that Keynes would have influenced the mind of Benjamin Strong of the Federal Reserve Bank of New York. Could he and Strong's successors between them have prevailed to mitigate the great slump of 1929–32? Then the Nazis would not have come into power. A fascinating speculation!

This passage is too naïve and ridiculous to call for additional comment.

The style is further marred by interrupting footnotes and side remarks to settle petty controversies in favor of Keynes. Perhaps more bothersome are the passages in which Harrod defends his own position on small matters that were somehow related to Keynes's life. He also uses the biography as another organ in which to air his own views. At more than one place we find nuances of *Are These Hardships Necessary?* Often when Harrod scolds his own government for not appreciating the American position in the recent postwar negotiations, it is really a partisan criticism of Labour's program and not criticism of fellow-countrymen, as such. A biographer need not be a man without any expressible personality, but he should make careful distinction between his own views and those which could be ascribed to his subject.

An appraisal of Keynes as a professional scholar and economist can perhaps best be seen through a consideration of each of his major publications or activities. I might characterize Harrod's appraisal by saying that he rates nearly

everything Keynes produced as a rare achievement of high quality. My own appraisal differs considerably.

The first phase is represented by *Indian Currency and Finance*, summing up his early work in the India Office as well as the training he received at Cambridge in monetary theory. This is a decent piece of work, thoroughly competent, but not something that will long attract scholars.

During World War I, Keynes served in the Treasury and, through his good work, rose to a respectable, responsible position in a short time. His training served him well, especially in giving him an understanding of financial markets, which later proved to be the source of his great personal fortune. It is also this government service which resulted in the position at Versailles and the subsequent protest resignation. The *Economic Consequences of the Peace* is brilliant and dramatic, but is it correct? In spite of Harrod's apologetic plea, I have grave doubts about the scientific merit of this volume. Are the statistical estimates as rigorous and sound as Harrod claims? In fact, Harrod praises Keynes as an outstanding empirical worker, but his statistical calculations in this and other studies do not measure up to current standards for carefulness and precision. Keynes's prognostication was correct that Germany would not pay the reparations finally assessed her; but this does not mean that she was unable to pay or that she could not have been forced to pay under different political circumstances. Keynes's volume did not necessarily swing a tide, but it did contribute its small assistance to a pile of sentiment in favor of softness toward Germany and the creation of a situation in which authoritarianism could not be readily checked. Harrod is certainly wrong in thinking of Keynes as the man who could have spared the world a Hitler.

The *Treatise on Probability* stems from Keynes's undergraduate training in mathematics and his keen interest in philosophy. Probability theory has made tremendous progress since Keynes's *Treatise*, but I should insist that this progress was in no way influenced by Keynes's work in that field. Today among some economists there is a revival of interest in the concept of subjective probability, but it remains to be seen whether this interest will provide fruitful developments. Harrod recognizes that the *Treatise* contributes little to the mathematics of probability; he looks upon it as a contribution to the foundations of probability. I should like to remind the reader that the basic work in the mathematical theory of probability carries the title (in translation) *Foundations of Probability*, by Kolmogorov. I find Kolmogorov's foundations far more secure than Keynes's. In sum, Keynes's ideas on probability represent a minority position among current workers on the subject and are not those for which we shall long remember his work. It must be made clear, however, that the *Treatise* does represent a rare accomplishment on Keynes's part, in that it shows convincingly his ability to do serious work in more than one field of endeavor, even though he did not make a sensational advance in probability theory.

The *Tract on Monetary Reform* is a good study for the times during which it was written. By itself it does not create revolutionary ideas, but it does foreshadow many later thoughts that will long be remembered. Harrod correctly points out the strain of consistency in Keynes's writings over more than three decades, in contrast to charges of incon-

sistency that have often been levied against him. Anyone participating as enthusiastically as Keynes did in affairs of the day during greatly varying economic phases will appear to be somewhat inconsistent in adapting arguments to fit the moment. The *Tract* shows clearly Keynes's consistent position on three things: (1) the need for freeing domestic economic policy from the blind dictates of international affairs, (2) the preference for inflation over deflation, and (3) the distinction between decisions to save and decisions to invest. Other than showing a step in Keynes's own development, the *Tract* has little more outstanding interest than as a tract on the political economy of the early 1920's.

The *Treatise on Money* was evidently intended to be a great work of lasting value, but turned out to be a double volume of confusion interspersed with some useful and some superb chapters on a variety of things. If it were not for the fact that this *Treatise* was followed by the *General Theory*, it probably would be of doubtful value today. Among its better points the *Treatise on Money* contains some often cited sections on index numbers, though by no means the best work on the problem; an interesting theory of bearishness, a prelude to liquidity preference; some inspiring ideas about the influence of investment activity on economic fluctuations; and a good practical discussion on money and banking. The parts that Keynes chose to emphasize are not the most satisfactory, especially the "fundamental equations" involving yet another rewriting of the quantity equations of money.

The *General Theory of Employment, Interest, and Money* is, without any doubt, Keynes's thoroughly justified claim to fame. This book represents his greatest achievement in economics, although it is best understood as the culmination of a long development which Harrod carefully traces for us. The *General Theory* is most important as a theory of the determination of the level of aggregate employment and output. It is also important as the basis for "Keynesian" economic policy. I should agree largely with Harrod's interpretation of the *General Theory* except for a point of emphasis which needs to be stressed once more, namely, that the doctrine of liquidity preference is more of incidental than central interest to the main theoretical argument.

In the past few years I have begun to wonder why Keynes's *General Theory* was so successful in gaining professional interest and whether the same ideas were not actually coming independently from other sources. At the theoretical level, others had some ideas about isolated aspects of economic behavior relevant to a self-contained, general theory of employment; but almost none, it appears, had the insight to bring all the relations together into one system. Keynes's spark of genius was just this. Recently, after having re-examined Kalecki's[2] theory of the business cycle, I have decided that he actually created a system that contains everything of importance in the Keynesian system, in addition to other contributions. Kalecki does not deal at all with liquidity preference and the interest rate; yet I believe that he has a theory of employment that is the equal of Keynes's. Kalecki's theory attracted attention for reasons largely unrelated to its revolutionary statement of the theory of employment, and he certainly lacked Keynes's reputation or ability to draw

[2] M. Kalecki, "A Macrodynamic Theory of Business Cycles," *Econometrica*, III (July, 1935), 327–44.

world-wide attention; hence his achievement is relatively unnoticed.

Some respects in which Kalecki's model is superior are that it is explicitly dynamic; it takes income distribution as well as level into account; and it makes the important distinction between investment orders and investment outlays. The dynamics of Kalecki's model attracted interest immediately. He did not go into the problem of unemployment equilibrium and the contrast with classical theory; indeed, his model contrasts with classical ideas on the possibility of achieving a stable solution. His consumption function is constructed with a unit marginal (and average) propensity to consume for workers and a value between zero and unity for the marginal propensity to consume of others. A more realistic view would have permitted the workers' marginal propensity to be less than unity and greater than the other marginal propensity—but surely this is a refinement.

At the policy level, the idea of the use of public works schemes, compensatory fiscal policy, and other means to stimulate the flow of spending during depressed economic situations seems to be nearly obvious. It is not entirely obvious because we know that the idea was seriously attacked in England by Keynes's opponents, who argued that public works expenditures would merely divert funds from private spending. At the trough of the depression Frisch[3] was advocating policies in Norway entirely like those suggested by Keynes. The Swedish anticyclical measures were similarly developed independently. In this country the New Deal was going ahead, on an intuitive basis, with its program of public works, easy money, and aids to consumer spending. Harrod takes up the question of the extent of Keynes's direct influence on the New Deal program and concludes that it was probably small, meaning that most of the same policies would have been carried out regardless of any contact with Keynes. In this connection Harrod argues that the New Dealers were going ahead by intuition, looking upon the direct employment offered by public works as the essential thing and failing to see that it was really the government deficit which buoyed up the economy. I dispute this point and argue that the intuition was not so bad as Harrod supposes. Subsequent formal analysis has shown that public works financed by balanced-budget methods carry a multiplier effect of unity, exclusive of any income redistribution effects. It is a very delicate matter of measurement whether the multiplier effect of deficit spending is sufficiently larger than unity to justify Harrod's argument.

An interesting extension of the work begun in the *General Theory* is provided by *How To Pay for the War*. This booklet dealing with the war finance problems of the early days of World War II extends the method of the *General Theory* to the analysis of inflation, to be contrasted with the depression-oriented development of the *General Theory*.

In World War II and the aftermath the bulk of Keynes's activity was confined to the Treasury office and is associated with two major items—Bretton Woods and the loan to Britain. In the latter negotiations Keynes showed, according to Harrod, remarkable abilities as a statesman, and I form the outsider's judgment that the appraisal is quite correct. Keynes's proposal for an international clearing union to serve the purpose

[3] R. Frisch, *Sparing og cirkulasjons regulering* (Oslo: Fabritius og Sønners Forlag, 1933).

of achieving exchange-rate stability without sacrificing the sovereignty of domestic economic policy is a scheme entirely in accord with his lifelong views. Harrod is overflowing in his praise for such a scheme, probably influenced to no little extent by his liberal view of life and consequent abhorrence of bilateralism, exchange control, and quantitative export-import controls—the prevalent devices of the period following the war. Keynes's proposal cannot yet be judged by the record because the institutions growing out of the Bretton Woods agreements have not begun to function in full force, but their accomplishments to date do not warrant lavish praise for the idea of the clearing union. Keynes's supposition was probably that international monetary management was not intelligent and that a way to ease Britain's postwar economic difficulties would be to design a clever scheme, a product of the intellectual aristocracy. Britain has had postwar economic difficulties, many of them having been foreseen by Keynes, but I fail to see that the cleverest possible international monetary arrangement, short of outright gifts, would have been of much assistance. The real problems have been to increase physical productivity in Britain, to reorient trade patterns with countries not participating in any international monetary agreements, and to retain as much as possible of a dwindling overseas empire. In spite of a heroic effort during his later years, following the outbreak of the war, Keynes will be remembered as an economist primarily for the *General Theory*.

The Keynesian theory of employment provides us with the necessary starting point for macro-economic analysis, but its use now is primarily as a pedagogical device. If one is to try to apply this theory for problems of scientific prediction —the necessary test for any good theory —he finds that he must build on the original formulation. The simple Keynesian theory is static and involves too few variables to be useful in direct application to problems of prediction. In the process of reworking the theory, one is never sure that a revision is merely an extension of Keynes's results or an essential change into a different or opposite kind of system. Much intended criticism of the Keynesian theory of employment has been on matters that are best viewed as extensions or refinements, with a very few being essential revisions. My personal opinion is that the basic theory stands up well in the face of empirical, statistical testing and that the future will see a more definitive treatment along this line.

Among more sophisticated critics of Keynesian economics, the central issue now concentrates on the questions of how wealth variables enter a macro-economic model and the consequent efficacy of flexible wages and prices in maintaining full-employment equilibrium. I think that these matters are not yet settled, but I adopt the view, on the basis of empirical studies to date, that some wealth variables have different effects in different sectors of the economy, some of the more pronounced effects being contrary to the objectives of Keynes's critics; that the magnitude of the effect of some wealth variables is not such as to invalidate Keynesian conclusions. Careless empirical reasoning is used frequently in citing the high postwar consumer expenditures in this country as evidence of the existence of the so-called "Pigou effect" (high real cash balances or wealth leading to high real expenditures) and the instability of the Keynesian consumption function. Concerning the high level of liquid assets in the hands of con-

sumers at the end of the war and the high levels of spending during the subsequent years, I should like to remind the reader of other pertinent facts, namely, that the large amounts of liquid assets were accompanied by relatively low stocks of consumer durables—another component of household wealth that often moves so as to lead to results contrary to those pursued by classically minded economists—and that the liquid assets were distributed more in favor of low-income classes than would be likely under a large number of other circumstances. If the Pigou effect is to have substantial importance, I should claim that it must be found among the lower-income groups. There is no assurance that classical wage and price flexibility is going to achieve a distribution of wealth or changes in this distribution which will bring about the automatic adjustments to full-employment equilibrium. In fact, there are many reasons why the mechanism may fail, in addition to all its deficiencies in the matter of building up a set of price anticipations which run counter to the classical scheme of things. The larger questions of what constitutes a valid, stable consumption function and whether such a function differs in an essential way from Keynes's are not yet settled matters, much of the voluminous writing on this subject during recent years being based on unsound empirical and theoretical analysis.

At the risk of falling into pitfalls that I ascribe to Harrod's exposition, I feel that I must cite one of Keynes's primary failings as his unsympathetic attitude toward modern econometrics and mathematical economics. This charge may seem surprising in view of Harrod's characterization of Keynes, at some points, as a mathematician; we know this to be the field of his early training. In spite of his background, I have long felt that Keynes failed to make the *General Theory* clear and that it was never fully understood until mathematically inclined economists formulated it in unambiguous, precise models. In fact, a rather simple mathematical development can easily be employed to push the theory further and free it from some of the major objections. Keynes really cannot have appreciated mathematical economics, to judge from his published comments on it, and one of his main positive statements in print praising Ramsey's article on savings[4] is surely a mistaken view. Ramsey's article is competent and interesting but hardly deserving of very special notice or comment that is not equally well due to numerous other articles in mathematical economics of the same period. Ramsey's methods and results are not especially novel. They certainly do not seem to be any less vulnerable than other research in this field that simplifies problems to bring out the essential theoretical content. In econometrics Keynes was even less well versed, his presidency of the Econometric Society in 1945 notwithstanding. His review of Tinbergen's celebrated study for the League of Nations was one of his sorriest professional performances. Many econometricians have remarked on Keynes's review with the comment that he simply did not understand the methods he was criticizing and failed to see at what Tinbergen was aiming, a type of comment Keynes frequently used to characterize his own critics. Ironically enough, much of Keynes's greatest support has come from econometric testing and application of his theory. This work will continue in the future and may play

[4] F. P. Ramsey, "A Mathematical Theory of Savings," *Economic Journal*, XXXVIII (December, 1928), 543–59.

a major role in perpetuating or extending the theory of employment.

Keynes's method of theoretical analysis can best be described as Cantabrigian; he was at times practically chauvinistic in his attitude toward work outside this university. The Cambridge method is often piecemeal and partial, attempting to study one part of an economy while putting the rest aside in a series of special assumptions. The *General Theory* is exceptional in attempting to construct a completely determined model covering economic activity as a whole. Much of Keynes's groundwork in this volume is, nonetheless, in the classical Cambridge style. Most mathematical economists shun the narrow Cambridge approach and try to encompass a degree of generality from the beginning of their analysis. Every man knows his own tools best, but I believe that Keynes's great insight would have pushed much deeper if he had adopted a less restrictive set of tools than those developed in Cambridge.

This review has concentrated largely on the parts of the *Life* having to do with technical economics, but professional readers will find the other parts both ample and interesting. It is difficult for me to judge Keynes's work or Harrod's interpretation in other areas with as great facility as in economics. In many respects, however, the reader can tell from concrete achievements the outstanding character of his other work. As a financier, speculator, and company chairman he built two fortunes, his personal wealth of about one-half million pounds, starting from nothing (even negative amounts in the early years), and the wealth of his beloved King's College. As an art and book collecter the record shows his holdings to be extremely impressive. The Arts Theatre in Cambridge will stand as a lasting memorial of his adroit efforts in organizing entertaining productions of drama and ballet. Keynes's literary achievements are only too well known, his mastery of prose showing to best advantage in skilfully executed biographical sketches or less technical essays. These are only some of the accomplishments that Harrod writes of at length in an interesting way.

Those of us who have worked on the economics of Keynes will highly value Harrod's scholarly achievement in dealing with the professional side of Keynes's life, realizing how extremely well he has done the job with the benefit of intimacy and access to personal papers. Future students of Keynesian economics—and there will be many—should first examine Harrod's volume in order to appreciate the underlying development.

The biography will be read and enjoyed by a wide circle of people for years to come. I find fault only with matters of detail and emphasis. I do not look upon each of Keynes's major efforts as a great contribution but find it necessary to get a view of his entire life in order to acquire a full understanding of those aspects that are truly great.

BOOK REVIEWS

Production of New Housing: A Research Monograph on Efficiency in Production. By LEO GREBLER. New York: Social Science Research Council, 1950. Pp. ix+185.

This is a fascinating book on the troublesome question of why our marvelous industrial machine does not supply us with houses as cheap and agreeable as the gadgets that stock them. The attacks on it have heretofore been made mainly by two groups: (1) kindhearted slum-clearers who are wont to shock the upper two-thirds with pictures of how the bottom third lives and then propose, through subsidies, to house it better than most of its social superiors (more pictures) and (2) theorists like Edgeworth, who, possibly after a bit of sidewalk superintending, argue that the problem is merely one of ignorance and poverty and that its solution lies in increasing the national product and in educating people to spend their enhanced incomes on better places to live. In recent years a new-type investigator has made the novel approach of examining the technical conditions under which dwellings are built. One of these is Leo Grebler, who has confined his efforts to disposing of the clichés heretofore given for answers and to raising many penetrating questions:

Do people want better houses enough to pay for them? If places to live were cheaper, might they not spend less on them and more on cars and television sets, insurance and clothes, education and parties?

Is housing really more expensive than other products of industry? Grebler points out that this has not been proved. The following is my comparison. An automobile selling for about $900 before the war weighed 3,500 pounds: 26 cents a pound. A four-room house, without basement and built to sell for $4,000, weighed 92,000 pounds (p. 12): 4½ cents a pound. The house cost only one-sixth as much per pound as the automobile, was built to last seven times as long, and on a weight-year basis thus cost no more than 2 per cent as much. To be sure, the dwelling was probably too heavy for its value; but so was the car. It was not gracious; but neither was the auto, especially after a few years' use. Anybody who dislikes this comparison is invited to come forward with a better one. Anyway, it illustrates some of the obstacles the housing analyst is up against.

Assuming for argument that the housing industry is less productive, could this be because it is less efficient or because its job is more complicated?

Has its efficiency been rising or falling compared to other industries? I will give A+ to any graduate student who investigates once and for all the legend of a decline in bricks laid per man-day from 2,000 at some vague time in the past to 400 at the present.

Can housing be produced cheaper by:

1. Changing the size of firm?
2. Breaking up monopolies and combinations?
3. Lowering wages through continuous operation and the guaranteed wage?
4. Modernizing building codes?
5. Aiming toward more prefabrication of houses and parts of houses?
6. Integrating the sprawling subcontractor setup?
7. Improving the organization of work on the site?
8. Introducing new materials?
9. Standardizing materials and making sizes interchangeable?
10. Making a different type of house and cutting down on the endless, and unintriguing, variety?
11. Reforming the financing system which destroys builders' independence and innovational bent?
12. Reducing the weight of houses, to cut down cost of transportation and make national production possible? (The operational radius of most prefabricators is 250 miles from the plant.)
13. Lowering interest rates?
14. Diluting labor skills through new processes, to make more use of unskilled and cheap labor?
15. Smoothing seasonal and cyclical fluctuations in demand, to get greater continuity of production? After all, most housing is produced at the high-cost stage of the cycle.

Grebler's avenues to greater efficiency are suggestive and leave one torn between optimism over the many possibilities for improvements and pessimism over the complications, which he makes no attempt to minimize. He gives no answers in this study but does supply a thoughtful discussion and the best catalogue of Ph.D. dissertation subjects I have seen. Many of his pro-

posed projects, however, are beyond the resources of a doctoral aspirant and would keep a host of foundations and industry research staffs busy for a decade or two. Indeed, as I read the list, the unhappy thought came into my mind: How many of the present population could be rehoused (even at present price levels) for what it would cost to answer his questions definitively?

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International Commodity Stockpiling as an Economic Stabilizer. By M. K. BENNETT AND ASSOCIATES. Stanford University: Stanford University Press, 1949. Pp. viii+205. $3.00.

The objective and scope of this book are sketched with characteristic explicitness in its opening sentence: "The purpose of this volume is modest: to further discussion of the question, how to attack the problem of avoiding wide swings in economic activity through concerted international action in the specific and limited sphere of commodity stockpiling."

This study is the outgrowth of a 1945 grant to cover costs of "an over-all evaluation of the Commodity-Reserve Currency Proposal," as advanced in Benjamin Graham's *World Commodities and World Currency*. The appraisal was undertaken as a staff project; and several years later, after several drafts, much revision, wide consultation, and final formulation by Professor Bennett, this book resulted.

It is neither a rebuttal nor an indorsement of Graham's proposals but rather a cautious, tightly limited, technically skilful exposition and analysis of international stockpiling as a contracyclical device. In the process the monetary and expansionist aspects of Graham's blueprint have been minimized, modified, or excluded; and the stockpiling and stabilization elements have been given central position. Thus, this book must be considered on its own merits and within its own frame of reference, not merely as an appendage or postscript to Graham's plan.

The book has twelve chapters and extended "Appendix Notes" (including somewhat rueful comments by Graham). In the first chapter, which is crucially important in this instance, the precise objectives and narrow limits of the study are stated. The deliberate restriction of the scope of the book is the basis of much of its impressive clarity, conciseness, and internal consistency; but it also limits its theoretical importance and policy usefulness. This is particularly true of one major limitation: the somewhat mechanical and textual examination of a set of proposals which must ultimately be scrutinized within a complete postwar political and economic context, both national and international.

Chapter ii further sets forth the framework of the book, for it contains a capsule statement of the Graham proposals in which stockpiling for stabilization rather than for expansion or monetary reform is highlighted. Chapters iii through ix then focus analytically upon the major aspects of the proposals as reinterpreted. Chapters iii and iv (apparently regarded as the central contribution of the book) are preoccupied with Graham's system of finance, which features monetization through the International Monetary Fund. Net purchases for stockpile would bring monetary expansion; each net sale would result in monetary contraction. In these chapters, written by Professor Edward S. Shaw, the proposed financing scheme is rejected as involving few advantages and as an incongruous, unstable, and vulnerable addition to the world monetary structure. In place of this system, a variant of the usual contribution and security issue plan is proposed.

The subsequent five chapters contain criticism, modifications, and elaborations of a less central character. These chapters include analysis and suggestions concerning such questions as these: eligibility of sellers, distribution of storage costs, management of stockpiles, buffer versus security stocks, calculation of market prices, circumstances appropriate to launching operations, target level and range of price stabilization, composition and weighing of a commodity unit, and the commodity-unit principle. In such spheres the technical level of performance is high, although implications are less thoroughly explored than are immediate operating problems. Chapter x is a sort of summary of chapters ii–ix in the form of a concise description of a properly modified Commodity Reserve System.

In chapter xi the risks of overaccumulation and excessive costs are held to be not excessive —on the assumption that the modified plan proves effective contracyclically. In the concluding and presumably climactic chapter, stockpiling (considered as merely one weapon in "the contracyclical arsenal") is cautiously and tentatively supported. It is acknowledged, however,

that such a program is of limited use, dependent upon complementary policies, vulnerable to nationalistic measures and international tension and disruption, and "not to be regarded either as an absolutely necessary contracyclical device or alone as a sufficient one."

If the decision to establish a contracyclical system of international stockpiling were to be made, this study would then provide invaluable technical assistance in implementing the new policy. If such a decision is itself the point at issue, however, the book is less useful. The political and economic preconditions for the success of such a proposed program are casually sketched in chapters vi and xii, but only very tersely and in terms that scarcely fit the post–World War II world. If a rather smoothly operating world economy is necessary even to launch the plan, the proposal can scarcely be placed on the agenda of any imminent international conference. If something less than such a peaceful and stable world would suffice, the minimum preconditions should be spelled out.

Although the book occasionally mentions the possibility of a clash between international stockpiling and domestic economic policies, this is actually a fundamental policy problem. For example, how would a stockpiling scheme of this sort affect—and be affected by—an aggressive domestic farm policy such as ours? Such questions may be no more "basic" than those considered at length in this book, but they are obviously relevant and merit more extended analysis than they receive.

A further limitation, both theoretically and in terms of policy, is imposed by the apparent lack of any systematic effort to appraise the order of magnitude of the effects of the program. We are assured that it is but one weapon in an arsenal, but we are not told whether it is a pistol or a bomb. This traces back to the failure of the book to assess the degree to which fluctuations in prices of "primary" commodities important in world trade actually cause or aggravate cyclical movements. Lacking this knowledge, we cannot even guess at the possible usefulness of a stockpiling scheme; and we cannot weigh gains against risk. The risks are specified, although minimized; but the gains are not.

Within its own carefully drawn boundaries, however, this book is clear, logical, dispassionate, and in the main competently done. It should be of genuine interest to scholars exploring such fields as stockpiling, resources, business cycles, and international economics. A companion study, less rigorously restricted in scope and more concerned with the setting, implications, and effects of such an international stockpiling program, is now in order.

C. Addison Hickman

University of Illinois

Economic Relations of India with Southeast Asia and the Far East. By N. V. Sovani. Bombay and London: Oxford University Press, 1949. Pp. 141. $2.00.

This study is primarily concerned with a rapid survey of the trade relations between India and Southeast Asia; the rest of the Far East is only incidentally mentioned. No attempt to provide a synthesis has been made, and the conclusions are mainly concerned with the difficulties which India will have in keeping her favorable trade balance with most Southeast Asian countries. Dr. Sovani notes the immense economic value of emigration for India, particularly the emigration of Indians to Ceylon, Burma, and Malaya, which has long since come to be regarded by many Southeast Asian countries as "an incipient Indian imperialism." While much of this emigration occurred under the aegis of the British, Sovani apparently expects it to continue. This expectation seems unrealistic: in Burma, Indonesia, and even in Malaya, with its large migrant Indian population, immigration restrictions have been established since the ending of the late war, keeping out all but the wealthier elements or the professionally trained. It rather appears that the Indian safety valve of emigration is gone and that India's economic future lies in increased productivity and industrialization at home. And this in turn will benefit her trade. Sovani notes how much of the jute industry has shifted to Pakistan since the partition. Since jute and cottons were India's chief export commodities, her trade has suffered in proportion. But the demand for Indian cottons has remained in most of Southeast Asia, particularly in Indonesia; and increased Indian production will yield considerable profit.

About half of the book is devoted to tables showing the trade between India and her Asian neighbors. In looking these over, one cannot help being struck by the diversity of exports India is capable of tapping in her commercial relations. Could this diversity not be usefully employed in the development of India's national economy? Sovani answers with a qualified "Yes,"

but he points out that two factors must be taken into consideration. First, the need for internal improvements in India's economic pattern, greater production, better distribution of wealth, and improved methods of land tenure. Second, India's national economy needs to be more integrated with the patterns of production in other Southeast Asian countries, so that definite types of export industries are built up which fulfil foreign needs. In this light an attempt to rejuvenate the jute and cotton industries may seem unwise, because Pakistan now has a head start on India in these two commodities. In addition, there is the problem of Japan's future exports, which before the late war, flooded the markets of Asia with cheap industrial commodities within the range of the average Asian's purchasing power. Therefore, a concentration on such items as coal, coke, fuel, livestock, fodder, household utensils, umbrellas, matches, and flashlights might not necessarily yield the expected returns.

Perhaps the most noticeable lacuna in Sovani's study is the absence of any mention of the character and internal structure of the trade. The need for capital is mentioned; just what is the extent of the capital resources in India, who controls it, and how has that control affected the trade? Dr. B. N. Ganguli, in his useful study of the *Reconstruction of India's Foreign Trade*, points to the proportional absence of any *petit bourgeoisie* in India with a resulting polarization of wealth and investment opportunity as a basic cause of the retarding of India's export commerce. While Ganguli may be overstating the case, it is certain that the structure of the trade channels in South and Southeast Asia needs to be examined more closely if any diffusion of wealth and any improvement in the national economy are to result. The many crippling restrictions on capital formation that exist in such countries as the Philippines, Burma, and to a lesser extent in India militate against the growth of indigenous capital assets and prolong the dependence of Southeast Asia on foreign investment. Much of the Southeast Asian trade today is still essentially of the peddler variety, involving only limited goods used in many transactions from market to market. Any national economy of the future would have to absorb the peddler in a much more co-ordinated framework of investment and production.

JUSTUS M. VAN DER KROEF

Michigan State College

Policy for the West. By BARBARA WARD. New York: W. W. Norton & Co., 1951. Pp. 317. $3.75.

This is a very able statement by the assistant editor of the London *Economist* of the policy of "containment" upon which the Western world is now acting. It should contribute to an understanding of what that policy is and what it implies and to a stronger faith in its ultimate effectiveness. May it be widely read and fully understood!

In the first part Miss Ward argues the case for "containment." Soviet hostility must now be assumed; "one thing only assuages it—the certainty of total control" (p. 20). Appeasement is unending, war initiated by us unthinkable, containment our only choice. That requires "sufficient military strength to deter the Russians from thinking they can manage a local aggression too speedily for effective counteraction to be possible" (p. 31), but it also needs a "social strength and cohesion that can discourage the Communists from trying out their favourite contemporary weapon—the fomenting of civil war" (p. 31). After describing the development of effective policies of containment in Europe—the Atlantic Pact and the Marshall Plan—and recounting the sorry story of Western policy in the East up to the opening of the Korean war, Ward states the spirit in which the policy must be undertaken. "We shall certainly fail unless our effort is at once sustained, calm, and supremely positive" (p. 81). Throughout the book the emphasis is on the positive: "We need to recover the sense of the great ideals and affirmations of Western civilization which a century of cynical rationalism, popular Marxism, cheapened science, and practical materialism have almost lost to our sight" (p. 85).

In the second part the economic strength of the Western World is assessed. The balance of strength clearly lies with the West, if the will is there. But it requires a readiness to face the cost. "There is a disturbing hint in British plans that the rate [of rearmament] has been fixed not on the basis of what is necessary but on the basis of what is the most that can be done without upsetting anything else" (p. 102). This attitude is not peculiar to Britain; Ward's further comment is worth emphasis: "It would be a poor service to a community to give it the instruments of better living—the schools, the hospitals, the industrial investment—but leave it without the means of defending these good things" (p. 102). Consideration is given to the

problem of inflation and the possibility of controlling it. Perhaps here too little is said about the danger of cutting the armament program to avoid inflation rather than undertaking whatever rearmament is necessary and then coping with the problem of inflation that the program creates. The emphasis is again on the positive: increase production, maintain employment.

Part III is concerned with the possibility of united action by the West. The problem of economic imbalance is examined, its roots deep in the past, its dangers clearly exemplified in the thirties. Recent "experiments in unity" are examined, and considerable optimism is based on them. But the policies must be expansive, not restrictive.

Finally, a brief fourth part, one chapter only, "Faith for Freedom." "Any human enterprise, even the smallest, needs a measure of faith. Men must believe that what they have undertaken can be carried through. They must believe that their partners will work with them loyally" (p. 299). But this is no more than the minimum. "The weakness of the phrase 'containment' is its negative and defensive ring" (p. 300). So we end on the positive note: "The Western experiment is really the most audacious and exhilarating that mankind has ever made" (p. 308). But it is not just a positive confidence in our "values" that we need; it is necessary to examine the weaknesses of our society and try to eliminate them. "We shall not defend it better for being so blindfolded in the conviction that all is well and there is no work for us to do. There are grave weaknesses and inconsistencies in Western Society; and these, more than anything else, give the Communists their opportunity" (p. 19).

V. W. BLADEN

University of Toronto

Wealth and Income: An Analysis of the Economic and Financial Systems of Australia and Great Britain. By BRIAN TEW. Carlton, Victoria: Melbourne University Press; New York: Cambridge University Press, 1950. Pp. ix+307. 21s.

This book is classified by its author as "an outline of the behaviour of a country's economic system as a whole"; and its two objectives are (1) "to describe the workings of the financial institutions of Australia and Britain" and (2) "to provide, in as simple and realistic a form as possible, the essential basis of economic theory required by an intelligent student of current affairs" (p. v). The "fundamental relationships" of that basis of theory are taken from Keynesian economics; and the author (correctly) believes the book to be "to a considerable extent unorthodox in its arrangement and exposition, and to some extent original in matters of detail" (p. vi).

Of the three parts of the book, "Part One is mainly concerned with those remarkable pieces of paper, bank notes, and with paper titles such as bills, bonds and shares . . . with paper wealth in its numerous varieties, how it comes into existence, what it is used for, and the terms on which one variety is exchanged for another" (p. v). "Parts Two and Three deal with the terms on which paper wealth is exchanged for goods and services, and the scale on which such exchanges occur. . . . In Part Three these questions are viewed from a particular aspect, namely in respect of the financial relations between the home country and the rest of the world" (p. vi).

This little volume is unique in two respects. First of all, the author has an extraordinary mastery over the English language. His limpid and expressive prose carries the reader along, without ever entangling him in mazes of reasoning or causing him to stumble over technical terms. All technical terms and concepts are introduced and paraphrased in clear and simple language. The second unique quality of this book consists in the author's treatment of the "fundamental relationships" taken from the works of the Keynesian school. He never hesitates to point out the conditional nature of those relationships; and he fleshes out the borrowed skeleton of theory with many descriptive passages, apt illustrations, and shrewd observations on the realities of both economic and political life.

The contribution which this book makes to economic literature is confined almost entirely to the two unique features mentioned above. Perhaps some theorists will be interested in a few points, such as the author's reasons for excluding changes in the interest rate from the factors which will determine the volume of "material investment" in the future (pp. 193–98). A book of this kind would be unsuitable as an introduction to economics, because it ignores too many basic economic concepts and functions. Nevertheless, it should be useful to economics teachers in two different ways: First, it contains many excellent descriptions of particular institutions

and concepts such as money, the London bill market, techniques for official support of the bond market, national income, the multiplier, etc. Second, it contains many obviously disputable arguments and statements which should be effective stimulators of class discussion or even student thought.

H. P. B. JENKINS

University of Arkansas

The Post-war Financial Problem and Other Essays. By F. W. PAISH. London: Macmillan & Co., 1950. Pp. 263. $3.00.

In this interesting little volume Professor F. W. Paish of the London School of Economics has gathered together the more important of the essays which he has written, chiefly in *Economica* and various British banking journals, over the last fifteen years. The essays, which range over a rather wide field, fall into two main groups. The first five deal with some of the problems, and especially with various aspects of the suppressed inflation, associated with the financing of Britain's post–World War II reconstruction efforts. The remaining seven—all written before 1941—comprise, among others, more scholarly papers on balance-of-payments adjustment theory, floating debt policy, the causes of gold-supply fluctuations, and the effects of taxation on economic incentive. For the most part, the essays fall gracefully into the category of "applied economics" and reveal at once the author's intimate familiarity with British financial practice and policy, his grasp of theory, and his ability to combine the two with fruitful results.

Like the majority of his fellow-British economists, notably Harrod, Meade, Robbins, Hicks, and Jewkes, Paish attributes to Britain's postwar inflation, imperfectly suppressed by all manner of controls, the primary cause of that country's economic difficulties and balance-of-payments pressures. The arguments advanced—of obvious relevance to the present debate on inflation control in our own country—are by now all too familiar. The experience of Britain, according to Paish, reveals only too strikingly the deleterious effects of suppressed inflation in maldistributing resources, falsifying the price system, weakening incentives, and restraining productivity growth. Writing against the background of the situation prevailing in 1946–48, he rejected the alternatives of open inflation or a more perfectly suppressed inflation and plumped instead for an elimination of the overall excess of aggregate demand to be accompanied by a gradual removal of controls. Unlike others sharing this general viewpoint, however, he was skeptical of the desirability of reducing investment (deeming it already too low), and, although critical of the ultra-cheap money policy of 1945–47, rejected a policy of monetary deflation and higher interest rates. To curtail the inflationary pressure, he argued, chief reliance should be placed on higher indirect taxation and on reductions in "unessential" government expenditures; sharp increases in direct taxes were ruled out because of their disincentive effects. It is not quite clear to what extent, if any, he would want to modify these conclusions in the light of subsequent, and especially post-Korean, developments.

Although the course of postwar economic developments in Great Britain will probably always be subject to conflicting interpretations, Paish has at least put his finger squarely and convincingly on an undeniably basic cause of the British malaise. But in his zeal to prove his case he seems to have considerably oversimplified an exceedingly complex problem. Some, with the present reviewer, will feel, for example, that he exaggerates the influence of the suppressed inflation and correspondingly minimizes the impact of other government policies and of the powerful structural changes wrought by the war in the British economy; that he places perhaps undue faith in the efficacy of the price system in the abnormal conditions of postwar Britain; and that he minimizes the political implications of his recommendations. Conspicuously missing, moreover, are any references to the possibility of reduced saving as a partial offset to the increases in indirect taxes which he proposes; nor is there any explicit recognition of a possible wage-price spiral stimulated by such increases.

Among the earlier essays are included Paish's outstanding paper on "Banking Policy and the Balance of International Payments," originally published in 1936. Although this paper is now clearly dated in the light of the subsequent work on the income theory of international adjustment by Metzler, Machlup, and others, due credit must be given to Paish for having been the first explicitly to introduce, and systematically to analyze the implications of, the concept of the marginal propensity to import. In a little-known companion piece entitled "Forecasting

Foreign Trade" (1937), written at a time when statistically derived import propensities and elasticities were not yet available, Paish develops a modest statistical technique for forecasting the imports of particular countries for short periods ahead. The remaining papers, which should be familiar to readers of *Economica*, call for no comment.

Altogether, the essays in this volume are stimulating and lucidly written and are likely to be of interest to a broad audience of economists.

ARTHUR I. BLOOMFIELD

New York City

The American Cigarette Industry. By RICHARD B. TENNANT. New Haven: Yale University Press. Pp. xxvi+411. $5.00.

This well-written study brings the history of the cigarette industry down to 1949. It reports that dissolution of the tobacco trust in 1911 seemed, three decades later, to have succeeded in its purpose of making the successor companies independent of one another and of stimulating aggressive competition among them. These effects have been intensified by subsequent judicial action, including the conviction in 1941 of several companies and their officers after trial in Lexington, Kentucky, on criminal charges of conspiring to establish monopoly.

The consequences flowing from these victories, however, have not been what earlier students would have expected. Newcomers have found it almost impossible to break into the industry. Only one nonsuccessor company has fought its way into the "big six," and it is still much smaller than any of the "big three." The number of producers has remained small. Competition among them has concentrated upon advertising. Profits have remained high until very recently; there is no assurance that they will remain at the lower levels to which they moved in the 1940's if the wartime taxes largely responsible for the reduction are loosened or if management becomes bolder as memory of the convictions of 1941 fades.

The principal reason why attempts to break up the cigarette industry have produced such limited results, Professor Tennant concludes, lies in the advantages the large firm holds as an advertiser. In this industry, he says, "advertising is a highly efficient competitive weapon which is somewhat erratic in its effects owing to the difficulty of finding an effective sales message. In general, large expenditures secure large sales. The leverage of large total outlays and the dependence of present on past sales mean that size can only be opposed successfully by size. The result is to encourage the large firm at the expense of the small and the existing firm at the expense of the outsider." In contrast, the markets for leaf tobacco and the technical processes of manufacture seem to offer nothing that favors the "very large" as compared with the "moderately large" company.

Despite the persistence of characteristics he finds socially undesirable, Tennant believes that the Sherman Act has exercised a wholesome effect upon the cigarette industry. In particular, it has eliminated the cutthroat competitive methods of the trust, and it has led to a great restraint in pricing during recent years. "The latent power of the Sherman Act is undoubtedly unpleasant for businessmen to live with, yet the act still appears to be, for all its untidiness, one of the most useful expressions of public policy."

As to further reform through intervention by the government, Tennant suggests three possibilities: (1) Some sort of regulation designed to prevent the reappearance of exorbitant profits in the future. (2) Limitation of each company's expenditure for advertising to the amount "a moderately large firm would choose to employ." (3) Substitution of an ad valorem basis for the present weight basis in the federal excise tax on cigarettes, the objective being to encourage the manufacture of low-price brands. None of these recommendations is worked out in detail or advocated very vigorously. Tennant himself seems to find them somewhat anticlimactic after his long, painstaking, and detailed study.

His analysis has drawn its factual data principally from the records of several governmental investigations and court actions. These permit him to trace the development of the industry in considerable detail through World War I, the short but severe depression of 1920–21, the prosperous 1920's, the prolonged depression of the 1930's, the New Deal, World War II, and the subsequent uneasy peace. In addition, he has made good use of important statistical and theoretical tools that were not available to earlier students of the industry. Notable among these are correlation analysis and the theories of monopolistic competition.

Although his subject is highly controversial, Tennant leaves few openings for disagreement with either his methods or his conclusions. He

has done a thorough job of assembling and analyzing his materials. He reasons well, and he writes well.

REAVIS COX

University of Pennsylvania

Hauptfragen der Wirtschaftspolitik. By ADOLF WEBER. Berlin: Duncker & Humblot, 1950. Pp. xii+374. DM. 15.

Adolf Weber, not so well known in the United States as his contemporary, Alfred Weber, has added to his massive output of half a century an attractive little volume which will be of especial interest to the economic historian and to the student of economic thought. In his Preface the author writes that he is making a kind of report on a life of scholarly effort in which the guiding principle has been to make economic theory throw light on practical affairs. To achieve this end, Professor Weber has selected for simultaneous presentation fifteen essays which have appeared previously as journal articles or, in a few instances, as unpublished lectures. The greater number of these brief studies were done in the decade of the 1940's. Each is preceded by a section containing "reminiscences" of the author, and this autobiographical material gives a rather impressive unity to the whole work.

Weber's type of analysis will not be satisfactory to the greater part of Anglo-American economists. The author is at pains to argue the necessity of models, of *Denkschemata*, without which the analyst must sink in a sea of facts. Yet he does not clearly understand, as did his countryman, Walter Eucken, that the propositions of economic theory do not necessarily say anything about conditions which actually exist. For Weber, theory must be at least partially descriptive of a real economy in a given time and place. He will only admit that mathematics may be of some use in exposition and that "pure theory" is essentially harmless. Although he would not go so far, it is with obvious relish that he quotes a remark of Gottl, who described "pure theory" as a "Güterzirkus mit Wert- und Preis-Akrobatik auf dem mathematischen Trapez mit dem homo economicus als Clown."

Running through this book as a recurring theme is the author's insistence that the main problem of public policy in the capital-poor countries of postwar Europe is the improvement of the economic lot of the working people. The hope of economic progress lies in the restoration of capital, the "dynamic" element in production. Yet the economics of Adolf Weber, which proclaims the nineteenth-century verities of classical German political economy, seems hardly equal to the task of bringing the hope to fruition. Monetary and fiscal policy must be sound in the orthodox sense. There is no theory of the determination of the national income. The reader may or may not agree with the adverse criticism of Keynes as it is presented in detail, but it is quite apparent that the new way of thinking about economic matters has not been accepted by the author or by many of his colleagues and followers.

Yet this is essentially a good book, a book well worth reading. The author's close acquaintance with three generations of German economists enables him to give valuable insights with respect to the thought of a heterogeneous group of social scientists, many of whom are almost unknown outside Germany. A necrological essay on Heinrich Dietzel is especially illuminating as an explanation of the particular development which German economics was to take in the twentieth century. As Weber passes, in the latter half of the work, to a consideration of the dislocations in Germany after two world wars, his command of the economic facts of life is as striking as the erudition evident in the first essays of the volume. Only the sections on money contain material that is possibly too familiar to require repetition.

The book is written in a lively, anecdotal style which makes reading it a real pleasure. Its literary quality makes it noteworthy in a day when economists generally pay little attention to the "style and manner."

ROSS M. ROBERTSON

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